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THE

MAGAZINE OF POPULAR SCIENCE,

AND

JOURNAL OF THE USEFUL ARTS.

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MAGAZINE

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VOLUME THE FIRST.

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THE

MAGAZINE OF POPULAR SCIENCE,

AND

JOURNAL OF THE USEFUL ARTS:

EDITED UNDER THE

DIRECTION OF THE SOCIETY FOR THE ILLUSTRATION AND ENCOURAGEMENT OF PRACTICAL SCIENCE, AT THE LOWTHER-ARCADE, LONDON.

PROSPECTUS.

In offering to the public a new Scientific Periodical, when several productions, somewhat similar in design, are already in circulation, it may be necessary to offer a few words in explanation of the general nature of our plan, and of the particular characteristics by which it is entitled to claim a distinction from other works of the same class.

The Periodical Scientific Literature of this country consists, in the first place, of the Transactions of Philosophical Societies; in the second, of several Journals, devoted to communications on General Science, or limited to its particular branches. With the former it is superfluous to say, we are not likely to come into collision; they are the standard receptacles of valuable and profound memoirs, in which are recorded the details of elaborate and original researches. Our Journal has no aim whatever at becoming the vehicle of such communications. On the contrary, it is a distinctive character of our plan to exclude all papers of this description; and for this obvious reason, that, great as the merit of such papers may be,—(nay, absolutely in proportion to the depth of their scientific character,)—they are not, and never can be, of a *popular* nature. They are as a sealed book to the *general* reader.

The second class of periodicals to which we have alluded, are generally conducted with reference to the wants and tastes of the *scientific* reader only, and are chiefly occupied with original communications; in which the details of experimental research, or mathematical analysis, are developed at length. It is doubtless from such memoirs, and the means by which they are permanently recorded, that the science of this country derives its main support; but such information, however valuable, is conveyed in a shape little attractive to the *general* reader, and not calculated to inspire an interest in scientific pursuits, or to explain the mutual bearings and relations of philosophic researches.

In making these remarks, it is neither our wish nor our object to speak in terms of disparagement of existing Journals; but merely to explain that, great as are their merits, our scheme is *essentially distinct* from theirs. We think it cannot be controverted, that *a general, connected, and, at the same time, popular view, of the actual progress and condition of the Physical Sciences, in a form accessible to the general reader, is still a desideratum* in our periodical literature.

In the numerous and valuable publications to which we have referred, we find (for example) detailed and minute statements of researches in Astronomy. Now these may be thought extremely abstruse, perhaps unintelligible, by the Chemist or the Naturalist; yet the Chemist and the Naturalist may both be very desirous to know, in a compendious abstract, what is doing in the Astronomical World. Again, the minute details of the laboratory may appear repulsive to the Mathematician or to the Geologist; yet they might be glad to be in possession of an accurate outline of the progress of Chemistry. Again, although complicated details of processes and results, valuable in themselves, and to those occupied in the respective departments, may be very uninteresting, perhaps incomprehensible, to the ordinary reader, yet the man of general information, though not devoted to this or that branch of Science, may be highly desirous to know what is going on throughout the region of research and discovery.

But, above all, the *youthful* disciple is naturally anxious to understand something of the advances which science is making around him; and in the benefit of which he looks forward with the sanguine expectation of one day participating. Hence the importance of providing an intelligible, and, at the same time, correct guide; one that shall place in its true light the progress and prospects of Discovery, divested of all false pretension, and check delusive and visionary anticipation,—thus pointing out the right track to which the aspirant after real philosophical reputation must confine himself, and opening to his view that promising field of research accessible only to the sober and well-trained follower of the principles of the Inductive Philosophy.

We have hitherto spoken of Science in an abstract point of view; but we beg it may be distinctly understood, that we mean to comprehend also its Practical Application to the Arts of Civilization and Daily Life. This indeed will be, to the larger class of readers, probably the most interesting portion of the information we are desirous of communicating; and will consequently, form a prominent feature in our plan. Indeed, emanating as this Magazine does, from a Gallery in which are daily deposited, and introduced to the public eye an immense number of practical inventions, it will be naturally directed largely to the *elucidation of the principles* on which they depend; our Exhibition and our Publication will go hand in hand, and each will be a reciprocal comment on the other.

We have already said that our main distinctive rule is *the exclusion of all elaborate original memoirs of scientific researches.* In their place, what we propose to do is, from time to time, to condense, into brief Essays, or Abstracts, such Accounts of the Progress of Discovery in the several branches of physical inquiry, as shall place, in a *connected* point of view, the labours of the different eminent individuals engaged in these inquiries. We shall endeavour to assign to each their relative importance; and, as opportunity may arise, shall comment freely on their merits, or faults, as they may present themselves, in the exercise of an impartial critical judgment. In doing this, it will be our especial aim to divest every subject, *as far as possible*, of all *technicality of language.* This, of course, cannot always be entirely accomplished; but we are persuaded that it may often be done to a great extent, with the aid of a few familiar and elementary illustrations, which, though *not new* in themselves, may yet be highly useful in rendering what is *new* generally *intelligible.*

There is also, we are persuaded, from what we observe of the present state of public opinion, great room for elucidation of the real claims which Science has upon public attention; considerable need for a due maintenance of its just pretensions; and for a powerful call upon its real advocates to vindicate its fame from unworthy and unjust aspersions. Its connexion with the general advance of mankind in Improvement, Civilization, and Rational Happiness, is a topic well worthy of more prominent discussion than it has yet received; and the importance of a due attention to Science in Elementary Education, ought to be insisted on to a far greater extent than has hitherto been the case: above all, the higher claims of Physical, as connected with Divine Truth, must be paramountly enforced.

Our plan, then, is to embody these and similar topics in the shape of what (borrowing a phrase from the Daily Journalists,) we may properly call a *Leading Article.* One, or more, of such articles (according to circumstances,) will occupy a considerable space in each number; and will be exclusively *original* productions, in which the writer will avail himself of facts and information from all quarters; but for the *combination* of his materials, and the *remarks* upon them, the writer himself will be responsible.

A *second* portion of our number is devoted to the bare matter of fact of Scientific Intelligence. Under this department we recognise for convenience of arrangement, four main heads. I. Mathematical and Physical Science. II. Chemical and Geological. III. Natural History and Physiology. IV. Practical Arts. Each of these will obviously be understood to embrace a number of subordinate branches: and for each facts," of which the experimenter boasts, but a confused mass of dry results, utterly distasteful to a capacious and inquisitive mind, bent upon the inquiry after truths of any real moment. Mechanics may teach a man how to construct a roof, and Chemistry to dye a coat*; but to suppose these worthy studies for a man who is anything above a carpenter or a dyer, is to degrade the character of the pursuits which befit a truly enlightened mind, or which can be considered as constituting proper topics to form essential parts of a course of liberal and sound education.

With a certain class, and that, too, a class putting forth peculiar and almost exclusive claims to the character of persons of liberal, gentleman-like, education, we know that there exists a very prevalent disposition to treat Science not only with indifference, but with contempt: a disposition to look upon it as involving pursuits of a minute and frivolous description, which can only be congenial to a very singular turn of mind, characterized no doubt by a petty kind of cleverness, at best extremely quizzical: a sort of genius which bewilders itself, and seeks to mystify others, with certain enigmatical speculations, extremely ingenious, no doubt, but quite beneath the notice of the man of sense and of the world. The profession of Science is represented as full of cant and charlatanry; and the philosopher and experimenter are regarded as doubtless exhibiting wonderful powers of astonishing the world by the display of their extraordinary discoveries, but classed with the thousand other daily prodigies which are exhibited for the amusement of the polite world; and the eminent experimentalist, and the distinguished astronomer, are stared at, and talked of, along with the unrivalled rope-dancer and the unparalleled professor of legerdemain or of cookery.

And, indeed, amid the various pretensions to Science, the numberless attempts (often surprisingly successful) to acquire a scientific name, and the variety of schemes and projects which dazzle the eyes of men by their philosophic assumptions, it is readily admitted by all who can look a little below the mere surface, that there does prevail so large a proportion of vain and empty affectation and quackery, as often to afford some reasonable ground for the suspicions and censures we have adverted to.

A shallow and superficial smattering of knowledge, joined with a complacent self-sufficiency, sometimes deludes its possessor (but more often the public) with a persuasion of his high philosophic endowments, and thence, either hurries him on in the pursuit of the most visionary theories in Science, in the one case, or of speculations equally visionary to the public, however beneficial to himself, in the other.

Ill-conceived and crude theories may sometimes, indeed, unluckily explode of themselves, or may be provokingly overthrown at the first application of sound experimental criticism; and the ill-qualified aspirant after philosophic fame may receive a salutary check from such experience. But if he be clever enough to shun the risk of such a catastrophe, he may, by due circumspection, rise to a reputation of philosophic eminence in the eyes of the world; and if he know anything of his business, it will be hard if he cannot turn it to substantial account.

---

* These are the reproaches cast upon physical Science (almost verbatim), by an eminent professor, whom we will not name, in a public academical lecture, now printed.

It is clear, that for such unblushing pretenders to make their way at all, there must exist in the public mind some disposition to look favourably on the claims of philosophy; but it is equally manifest, that there exists a great want of well-informed and correct judgment to discriminate between the pretensions of the quack, and the sound teacher,—between the qualifications of the loud but shallow declaimer, and those of the retiring but solid investigator. The class of works on these subjects which acquire popularity, and obtain a wide circulation, may perhaps be taken as no unfair test of the tone and character of the public taste and judgment. And if so, we feel compelled to state our unequivocal opinion, that that taste and judgment greatly need not only the occasional corrective of a scientific censor, but the more steady and salutary influence which will arise from the general diffusion of a wholesome kind of scientific information; advantages which it must be confessed have been but very sparingly supplied hitherto, but which it will be our anxious desire to furnish.

In some subjects, it is true (especially those which from their nature hardly admit of much diversity of representation), such as Astronomy, Mechanics, &c., the authority of great names has maintained its ascendency; but in others, the case is very different. If we take only as an example the single science of Geology, it is perfectly surprising what a mass of almost incredible absurdity and ignorance is swallowed by a large portion of the public, while the sober researches of really scientific inquirers are disregarded, or condemned as visionary and even dangerous.

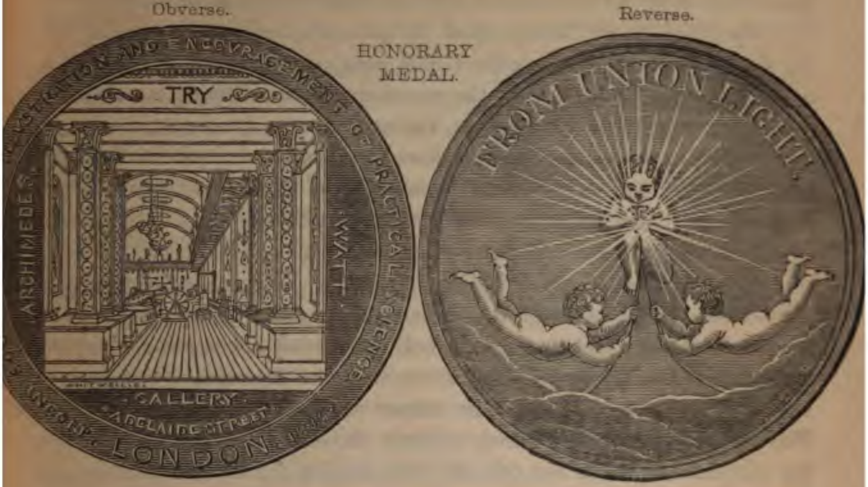
Even within the more peculiar precincts (as it were) of Science, we continually meet with exemplifications of the same deficiencies and absurdities. In proof of our assertion, we could cite many instances of the childish trash which often passes current as philosophical speculation, in the country of Newton, in the nineteenth century, in works sustaining a scientific reputation, and under the sanction of names really eminent in the philosophical world. A very few examples may suffice. One of the most able and justly distinguished naturalists of the day, has seriously advanced the theory, that the races of enormous reptiles, whose fossil remains occur in some of the older strata, are still existing in vast subterranean abysses. Another writer, in a grave philosophic paper, affirms that a live toad has existed shut up in a cavity without outlet, in a rock, ever since the creation. Philosophers without number, persist in believing, that because the sun's rays heat bodies in proportion to the darkness of their colour, therefore all other kinds of heat do so. A long theory has lately appeared, grounded on the principle that heat radiates through water, a fancy which, it seems, not even the clearest evidence can dispel in some minds. There are still to be found those who claim the title of geologists, and yet deny the clearest inferences from known facts in proof of the immense length of time during which the different formations composing the earth's crust have been successively deposited and elevated.

It would perhaps scarcely be credited by those who have not, as we have, had repeated occasion to witness it, that even at the present day there are vast numbers of aspirants after the glory of squaring the circle; that not many months since, a highly respectable and even learned individual, actually made a voyage across the Atlantic, for no other purpose than to communicate such a treasure to the British mathematicians, as a solution of this famous problem, which ended in establishing, by the clearest demonstration, that 49 are equal to 50! Mathematical treatises are published, professing to place some of the most keenly contested questions of first principles upon an entirely new and perfectly satisfactory basis; which, on examination, have turned out little more than servile copies of the errors and obscurities of their predecessors. We could add many more examples, but we forbear,—at least for the present. We must pass on from remarks, intended to be merely introductory, to the more substantial matter of our work. Meanwhile, we think it will now be allowed that we have made out our case; that after what we have said, the necessity for such popular views of the state and progress of Science, as we here purpose to supply, and which we trust are sufficiently explained in our Prospectus, will be generally apparent.

We have observed, that if by some the advance of Science is made matter of severe censure, and by others of ridicule, it is because its real nature and pretensions are totally misunderstood. If there be not generally evinced a due sense of its importance, this only shows the necessity for urging its claims. If, again, superficial pretensions are palmed upon the world for substantial philosophical attainments, it is because the world are too little able to discriminate them; it is from want of due exposure, and the adoption of adequate means to enable the public to become better judges of such pretensions. In these respects, then, the existing state of things is far from being so favourable as might be supposed, to the substantial advancement of Science: and this consideration will afford a full justification of the assertion, that we still need the application of judicious measures for the encouragement, diffusion, and defence, of real Science, and the detection of false pretensions; and a considerable augmentation of the powers at present wielded by the scientific periodical press.

We thus clearly evince the necessity which exists for the discharge of the united offices of the advocate and the censor,—of the vindicator of true philosophy, and the exposer and castigator of that which falsely usurps its name. The cause of real Science still needs an advocate: not a mere blind, unqualified panegyrist, always obtruding and upholding the claims of philosophy to admiration and pre-eminence, but a discriminating, impartial, and sober supporter of its just pretensions, in opposition to the narrow, illiberal, and ignorant aspersions of one party; and a determined and fearless critic, to expose all false pretensions raised under its name, and all pernicious errors with which the abuse of it may be associated, by another. The state of public opinion and prejudice to which we have alluded, imperatively calls for such an exposition of the real character and objects of physical philosophy as shall vindicate its rank among the worthiest and most ennobling of human pursuits: while the perversions of it in visionary speculation, and the abuse of it to promote more dangerous error, require the severest castigation of the critic. Such defence of the true claims of Science, and such exposure of its abuses, it will be one of our main objects, from time to time, to supply; such an extended dissemination of a sound knowledge of its principles as we have recommended, and such aids at once to the cultivation of a taste for its pursuits, and the formation of a correct judgment on their character, is precisely what is the essential part of our design to provide.

GALLERY OF PRACTICAL SCIENCE.



Obverse.

HONORARY MEDAL.

Reverse.

Description and Intention of the Medal of the Society for the Illustration and Encouragement of Practical Science, London.

Obverse:—The vestibule of the Gallery of the Society; beyond and through which is seen the gallery itself. The inscription on the frieze is meant to urge the speculative and the young to action, and the inventive to proof. The names of Archimedes and Watt are inserted in the field as the most eminent in Practical Science; the first, among the ancients, the last, among the moderns. *Legend:*—Society for the Illustration and Encouragement of Practical Science. Incorp. London, 1834, and the locality of the gallery.

Reverse:—An emblematical design, representing the Means and the Aim of the society. The Means—association; and the Aim—advancement and extension of scientific knowledge. The genii of Chemistry, Pneumatics, and Mechanics, are combining and producing the cal-oxy-hydrogen light, for which the gallery is so pre-eminent. Chemistry and Pneumatics are supplying the gases, and Mechanics is presenting the rotatory cylinder of lime. *Legend:*—"from union—light;" an important truth, physically and intellectually.

This medal, when struck in silver, is presented to the president and secretaries of all similar foreign associations, on their arrival in this country; to be by them conveyed to their respective societies, as an expression of congratulation by the English society to their foreign brotherhoods. The medal, struck in bronze, is also presented to every member of such foreign societies, who may visit London. The possession entitles the bearer to free admission, at all times, to the gallery.

The obverse is used by the society as their common seal; and as will have been observed, the reverse is inserted as a distinctive mark in the title-page of this Magazine.

It may be expected that, in this our first number of the Magazine of Popular Science, and Journal of the Useful Arts, some account should be given of the Institution from which it emanates.

The Gallery of Practical Science was projected in the Autumn of 1831, by a few individuals desirous to promote the intercourse between the cultivators of abstract Science, and persons engaged in its practical application; to illustrate scientific subjects in a manner at once interesting and instructive; and to afford to discoverers in philosophy, inventors, improvers of inventions, manufacturers, and individuals possessing interesting objects of virtu, the opportunity to bring before the public their discoveries or works of art, in an attractive and inexpensive manner.

The finishing and fitting-up of the premises occupied the period till the 4th of June, 1832, when the rooms were opened in the evening to a numerous company invited for the occasion: on the following morning the gallery was opened to the public. The number of visitors contributing to the support of the institution, was found gradually to augment, and the original proprietors were thereby encouraged to extend the basis of the establishment; and the most secure means of effecting that desideratum appeared to be a Royal Charter of Incorporation.

The late Thomas Telford, Esq., the most eminent engineer of his age—whose name will endure longer than even the numerous works of his construction, which have improved whilst they adorn our country,—and Francis Giles, Esq., civil-engineer, were the first individuals to join the original projectors; and in answer to their united petition, his Majesty was graciously pleased to grant the charter, which incorporates the shareholders under the title of "The Society for the Illustration and Encouragement of Practical Science."

Authority is given by the charter to divide the capital of 20,000*l.*—consisting of 4,000*l.* in money, and 16,000*l.* invested in the premises and philosophical apparatus placed therein—into 400 shares of 50*l.* each. It also confers the privilege of raising a further capital to the extent of 20,000*l.*, in similar shares, should the proprietors, at a general meeting, specially convened for that purpose, so determine.

All contracts are to be made under the corporate seal of the society, which, with the properties and affairs of the institution, is entrusted to the management of a council, consisting of not fewer than four, nor more than fifteen of the proprietors. The accounts are required to be made up and laid before the proprietors, at least once in every year, when dividends of the profits may be declared.

It must be obvious, that where the arduous task is undertaken of accommodating a public exhibition like the present to the various tastes of a mixed assemblage, there will, of necessity, exist some points which shall be highly attractive to one class of visitors, whilst they will be regarded as comparatively unimportant by others. Thus the Persian rope-dancer, which, with its fairy-like music and elegant movements, is a never-failing source of admiration to the young, may, by others, be held in light estimation; unless, indeed, a love of science shall lead them to examine and inquire into its ingenious and elaborate mechanism. So also with the automaton juggler; there are, however, some other marked features, some central points, as it were, of attraction, deserving especial notice; and to these we shall briefly advert.

We will commence with the series of Magnets, which are prepared for the daily illustration of some of the appearances and effects of electro-magnetical and magneto-electrical phenomena. The *Electro-Magnet*, possessing neither Electricity nor Magnetism, until excited by a very small voltaic battery; it then instantly acquires an enormous power of suspension: on destroying its connexion with the battery, it becomes again unable to support a grain. Another of the same kind, and called the *Ferro-Electric Sphere*, arranged to show the true cause of the Earth's Magnetism. A third, is a *Self-acting Electro-magnetic Machine;* in this also Electricity excites Magnetism, producing motion in a spindle, and by an ingenious contrivance, which effects an alternation in the poles, continues this motion, and gives out an uninterrupted succession of sparks and shocks for an indefinite period. The *Magneto-Electric Machine*,—in this combination Magnetism produces Electricity. This instrument was made in consequence of Professor Faraday's important discovery, that electricity could be obtained by means of magnetism; and was the actual one from which the first spark was first produced and seen in England. It now exhibits the spark most brilliantly and incessantly,—gives an intolerable shock,—decomposes water,—ignites and fuses platinum-wire, &c.

There is also an instrument which belongs to a department yet scarcely explored by scientific research, and one which we fear is too often passed unheeded; we allude to an apparatus for showing the Compressibility of Fluids by means of hydrostatic pressure which can be produced in this machine to the unprecedented amount of 30,000 lbs. to the square inch.

The celebrated Steam-Gun is too well known,—and the unceasing interest it excites, far too generally admitted to require any particular notice. We shall, therefore, merely observe, *en passant*, that a new barrel has lately been added, which increases the power and the precision of the instrument.

The unique engine for showing the Combustion of Steel, is also well worthy the attention both of the curious or the general observer; although the almost inconceivable velocity with which the wheel rotates, may, naturally, at the first moment excite some alarm.

Amongst the philosophical apparatus possessed by the Society, may be mentioned a Lens or Burning-Glass of nearly four feet diameter. A Cal-oxi-hydrogen Microscope of great power and unrivalled splendour, constructed by Cary, with various consecutive improvements made under the superintendence of the Society; and a Laboratory—intended to facilitate the advance of science and practical knowledge.

The Society receive for exhibition—Models of Inventions—Works of Art *and Specimens of* Novel Manufacture, subject to immediate delivery in the event of sale, or return on demand; *free from any charge whatever to their depositors.* It also affords every facility for the practical demonstration of discoveries in Natural Philosophy, or of any new application of known principles to Mechanical Contrivances; reserving only to the Council, the right of determining whether the productions offered are suitable to the Institution.

We shall, in future Numbers, refer to a class of apparatus, for illustrating one of the most attractive branches of Natural Philosophy, namely, Optics; but we have already exceeded our limits, and must not now dwell on them: still less can we advert to the numerous Minerals, Fossils, Paintings, and other objects of Arts, or to the extensive variety of Nautical, Mechanical, Architectural, and other Models. Indeed, it would be foreign to our purpose to enter into any detailed account of the objects in this interesting Collection: this comes more especially within the scope of the Catalogue, in which the Principles of Science, connected with the various models referred to, are familiarly and concisely defined. Our only endeavour has been to throw a cursory glance over its origin, general features, and peculiar character, to bring under the notice of our readers its objects and its tendency, and to show that it is what it professes to be, a GALLERY FOR THE ILLUSTRATION AND ENCOURAGEMENT OF PRACTICAL SCIENCE.

The Society, anxious to extend the utility of the Establishment to the utmost possible degree, directed by an Order of Council, in September, 1835, that a Circular be addressed to all "the scientific and learned Institutions in the empire, proposing to furnish them with the descriptive catalogues of the Establishment, free of expense; inviting the communication of interesting objects for public exhibition; and requesting any suggestions that the local knowledge of the Members of such Societies might enable them to make for the advancement of the Institution, and the instruction and gratification of the Public."

Number of persons who visited the Gallery in 1835, exclusive of annual subscribers, proprietors, proprietors' admissions, and free admissions to artists, depositors, and men of science,—80,375.

ON THE NATURE, EVIDENCE, AND ADVANTAGES OF THE INDUCTIVE PHILOSOPHY.

I.

THE Inductive Philosophy stands forth as the distinguishing boast of modern intellectual advancement, and the most prolific source of innumerable advantages,—mental, moral, and physical. It has opened the path now universally recognised as alone leading to the correct interpretation of Nature; of that stupendous order of varied existence, and incessant activity of causation, with which we are surrounded and filled. It is justly characterized as a method framed in conformity to experience; and stands essentially opposed to those artificial systems of former ages, which were but the vain chimeras of minds bewildered in the obscurities of verbal mysticism, or deluded by the conceits of gratuitous hypothesis, systems which cramped all energy of thought and invention, and fettered all freedom of opinion and discussion. By a combination of vague and unmeaning abstractions, involved in a pedantic jargon of empty terms, the scholastic disputants thought to settle the order of natural causes, and determine what *must be* the character of physical laws. From a few abstract, and those hardly intelligible, arbitrary positions, they affected to advance, by the mere subtilty of their reasoning powers, to a comprehension of the entire system of the material universe.

But the appeal to experiment and observation, and the high and pure physical philosophy inculcated by Bacon, and practically followed up by Galileo, Newton, and their successors, soon established the dominion of principles, at once more correct and rational, and better suited to the limited range of the human faculties. By the humble unpretending path of the inductive method, all the great triumphs of physical discovery have been achieved; by a steady adherence to its principles, can we alone expect the further extension of natural knowledge; and so long as they are adhered to, we can assign no limit to the progressive advance which may be made. And minds duly impressed with the sublimity of those inquiries which the contemplation of nature suggests, will easily be led to recognise the truth and value of the inductive method. They perceive, in reference to observation and experience, an appeal to the sole testimony of nature: they would interrogate her in her own language, and in the replies to those interrogatories, afforded by experimental results, acknowledge the only real and legitimate authority by which the comprehensive truths and laws of the material world can be successfully elicited and established; and by which simplicity and order are educed out of the vast mass, and (as might appear) inextricable complexity of accumulated phenomena.

It is not, however, our intention to go on with a mere string of encomiums on the Inductive Philosophy. Its claims are now generally allowed, and its praises held forth; still, not unfrequently, its advocates and encomiasts entertain very indistinct notions of the real nature of the system they support. At all events, in the study of Natural Science, it must be allowed *to be a branch of no small* interest and importance to examine *carefully the nature of the reasoning*, and the general grounds of the *evidence*, by which experimental laws, and physical truths, are substantiated; and this, in fact, is what is meant by the expressions so commonly used by writers and teachers, "the inductive method," "the inductive logic," "experimental evidence," and the like. Such phrases, perhaps, pass current by the influence of custom; but it will be both an interesting and profitable topic of investigation, if we analyze their meaning a little more closely, and endeavour to exemplify and elucidate the nature of our convictions and inferences, in these branches of knowledge, the degree of certainty of which they are susceptible; and the sources of failure and error to which we are most exposed in the prosecution of physical inquiry, without some well-grounded principles of this kind as our guide.

We propose, therefore, to devote, in several successive Numbers, a small space to the continued portions of an Essay on the nature, use, and advantages of Inductive Science; and in following out this design, without restricting ourselves to too formal and technical a method, our first object will be to examine briefly the *real nature* of the inductive process: and to illustrate, by familiar examples, wherein the most essential and characteristic features of *inductive* evidence consist, as distinguished on the one hand from the mere evidence of our senses, and on the other from demonstration.

We will merely observe for the present, that although this is a part of the subject properly belonging to the province of our Journal, yet we are aware it may be imagined likely to prove abstruse and dry; we therefore beg to assure our readers that we have no intention to lose ourselves and disgust them in logical subtilties and metaphysical abstractions: such we trust will be far from the case. We shall merely entreat the patience of the student while we lay down a very few elementary distinctions, which we deem essential to a right understanding of the subject, and shall then proceed to those more general deductions, which will receive abundant exemplification by references to the actual progress which Science has made, or has failed to make, precisely as these great rules of inductive evidence, (which we may, without much impropriety of language, term the logic of nature,) have been observed or disregarded.

ON THE TIDES;

AND THE RECENT PROGRESS OF OUR KNOWLEDGE RESPECTING THEM.

AMONG those grand subjects of contemplation and inquiry which the natural world presents to us, there are perhaps few at once more calculated to excite our curiosity and admiration, and of more practical interest, than the phenomena of the TIDES. To witness, from day to day, at a certain regular succession of hours, an enormous body of water advancing by slow degrees, defying all barriers which may be opposed, until it reaches a certain elevation, and then as regularly falling and retreating; its very apparent irregularities being soon found to conform themselves to regular periods; and all this without any apparent cause acting to produce it;

and going on with unceasing regularity not merely in one place, but all over the world. These, truly, ought to be among the phenomena most powerfully claiming our attention, and prompting inquiry into their causes and laws. But further, when we look at the great practical importance of the subject,—at the immense utility of these vast fluctuations in the economy of nature, as well as in reference to the purposes of human arts, their importance to the navigator, and their influence on commerce, it becomes, beyond question, manifest how large a share of interest the subject ought to call forth, and with what care it would be reasonable to expect it should be investigated. Yet we shall find that, until of very late years, it has been singularly ill attended to. The ancients had of course noticed the phenomena, and some philosophers had thrown out vague surmises that the cause of it was due to the action of the sun and moon: but there their speculations terminated. On the revival of learning and science, we find several philosophers hinting at the nature of the cause of the Tides, though no one gave anything like a satisfactory explanation till Newton. He perceived at once that the phenomenon was, at least in its more general features, a simple consequence of his principle of universal gravitation acting between the sun, the moon, the solid earth, and the waters of the ocean.

The great characteristic of gravitation is an attraction, or drawing, of every particle of matter in the universe towards every other particle; aggregated masses, drawing of course in proportion to their masses; and the intensity, or degree in which they draw each other, increasing exactly in proportion as the squares of the numbers measuring their distances, *decrease*. The sun, being a large mass, but at a great distance, attracts the moon, earth, and water; the earth attracts the moon, which is revolving at a certain distance about it, and the water, which is retained in a certain degree of adherence to it, though in a certain degree free to move. The moon attracts the earth, as the larger mass, more than it does the water, which is less. If the globe were entirely surrounded by water, the portion of water next the moon would be attracted by the moon, as being nearest; the body of the earth next, as being the largest; and the mass of water on the other side least, as being smaller and also more distant. Thus there would be a rise or swelling out of water on the side towards the moon, and another rise or swelling out on the side away from the moon: the first mass of water being drawn away from the earth; the earth being drawn away from the second. The sun, being at an immensely greater distance, has a less powerful action, but of the same kind. As the earth revolves in twenty-four hours on its axis, any one point on its surface is brought once under the moon, and once into the position opposite; and at each position, experiences the rise or protuberance of the waters just spoken of; in other words, two daily tides, or high water twice in every twenty-four hours. Such is the elementary conception of the matter which was developed in a general way by Newton. But though these general principles are sufficiently simple, and require nothing further for their comprehension than a clear notion of the nature of the law of universal gravitation, yet, for the complete mathematical analysis of the problem, when pursued into all its details, a number of far more abstruse considerations are requisite to be made out and attended to.

This attraction of the waters towards one part, would then create, as it were, a swelling or protuberance along that meridian of the globe which was at the time under the influence of the cause producing it. Two such *ridges*, as it were, would exist at opposite sides of the globe. This ridge, or crest of the water, would be simply one enormous *wave*, extending in length from pole to pole, and in breadth, a quarter of the earth's circumference: this is called the tide-wave. Still, supposing the globe uniformly covered with water, these waves would travel round the globe, each maintaining always the same relative position and direction; or, in other words, any one spot in the ocean would, twice in the twenty-four hours, be elevated to the summit of the ridge, and twice depressed into the bottom of the hollows between the ridges. The tide-wave would move uniformly from east to west; and its direction coincide with the meridian, if the sun and moon were always in the equator. On this hypothesis, the phenomena would present themselves with undeviating regularity. The theory would be easy to investigate upon the principles of gravitation, joined with those of oscillating fluids. But this is manifestly far from the actual condition of things.

The globe is not covered with water; nor are those parts which are so of uniform depth. The sun and moon do not move in the equator; but the former in the ecliptic, and the latter in a plane very little inclined to it. This introduces complexity into the phenomena, and great difficulties into the application of the theory of gravitation to them. Yet a certain modification of the simple conditions will still be preserved. A line drawn through all those points which are on the crest of the tide-wave at the same instant, is named by Mr. Whewell a *co-tidal line.* In the simple case above supposed, this line is a meridian of the globe; but in the actual case of nature it is broken, contorted, and reflected into a variety of new and irregular directions. Still they will preserve a certain degree of symmetry; in the wide expanse of the ocean something like uniformity will be maintained: but even here, it will be readily seen, that many causes must be taken into account, as operating on the course of the tide, and the form which its ridge will assume in its progress.

"The tide-wave," (as Mr. Whewell has admirably described it,) "by its motion, brings high water and low water to any place at the time when the elevated and depressed parts of the watery surface reach that place. The co-tidal lines, for successive hours, represent the successive positions of the summit of this wave: and if we suppose a spectator detached from the earth, to perceive the summit of the wave, he will see it travelling round the earth in the open ocean once in twenty-four hours, accompanied by another, at twelve hours' distance from it; and both sending branches into the narrower seas: and the manner and velocity of all these motions will be assigned by means of a map of co-tidal lines."

If, for example, we consider the tide-wave advancing from east to west, it will be evident that the continents of Africa and South America will act as immense dams, which will arrest the progress of the tide, *and change its direction.* Coming directly against those shores, and there being stopped, it will take a course along their length: it will run round the Capes in which they terminate. These broken streams, as it were, will again meet with others occasioned by the like obstructions of other lands: their combinations, either conspiring, or opposing, will produce a vast variety of irregular and complex effects, often apparently at variance with any uniform law; which will again be still further modified by the extremely different amount of the depth in different parts of the ocean, and the form and structure of the bottom. Similar effects will result, with a much greater degree of complexity, in smaller seas, more broken and interrupted by promontories and islands.

Such would be the first rudiments of the theory; but Newton, in the developement of the vast system of universal gravitation, clearly saw that it would be utterly in vain for him to attempt following out the principle into all the varied and complicated results to which it led. He contented himself, and wisely, with verifying all its great leading points, and leaving the minuter details to his successors. Thus in regard to the Tides, he proceeded only to the general demonstration of their dependence on the attractions of the sun and moon, and showing in a very general way, what course the flow of the tide would take in its daily progress round the globe; but the explanation of all the details of the subject was bequeathed as a legacy of research to his successors. That is to say, all the apparent irregularities in the progress of the tide-wave, all the variations in the time of high water at different places, all the particular effects of the obstructions occasioned by the varied forms of continents, and the changes in the depth of the sea, were to be examined and described; and then again the theory was to be brought to bear upon them, so as to show whether it would afford a satisfactory explanation; and thus the whole series of phenomena, not only in their grander features, but even up to their lesser details, be all susceptible of explanation on the one comprehensive and pervading principle of *gravitation*. To this task then, Newton's successors addressed themselves. The completion of the Mathematical theory was the first object of their attention. The Academy of Sciences at Paris, proposed a prize on the subject, in 1740. Bernoulli, Euler, and Maclaurin, contended for it; and each produced essays of such singular merit, that the prize was divided between them. They did not succeed, however, in completely delivering the mathematical part of the subject from all its difficulties: but these arose from the imperfection of then existing mathematical processes. At a later period, Laplace applied his gigantic analytical powers to the subject, and in a great degree removed those difficulties. The investigation, however, was not yet strictly complete, or direct, though reduced to a form, in a great measure, applicable to the purposes of comparison with observation. The analysis was carried to greater perfection by MM. Fourier, Poisson, and Cauchy, about the year 1815. And even in 1832, the Academy of Petersburgh proposed as a prize-subject the complete and exact solution.

Before this time, the subject had attracted the attention of that eminent mathematician and zealous cultivator of Physical Astronomy, Mr. Lubbock; and at the meeting of the British Association for the Advancement of Science, in 1832, that gentleman produced, at the request of the association, a masterly report on the present state of our knowledge on this

curious and important subject. He found that theory, though, as we have stated, not brought to a state of absolute mathematical completeness, was much further advanced than our knowledge of the facts from observation. In this, indeed, the deficiency was great and singular. The question had suggested itself to the penetrating mind of Bacon:—What was the connexion between the tides in different parts of the world? and, in particular, whether the high-water extends across the Atlantic, so as to affect, contemporaneously, the shores of America and Africa? or whether it is high-water on one side of this ocean, when it is low on the other? Even at the present day we are hardly able to answer this question with accuracy: but it seems probable that the former idea is near the truth.

The extreme practical importance of a correct knowledge of the tides on coasts and in harbours has, in many places, led to the prosecution of some sort of regular observations, though mostly of a very rude kind, to determine what is called *the establishment* of particular ports. This means the interval of time after the new and full moon passing the meridian, at which it is high water there; from this the time of high water on other days is known from the age of the moon. At most ports, in commercial countries, some rude data of this kind have been long determined; and it is a singular circumstance, that even up to a very late period, determinations of this kind, and rules for the calculations deduced from them, should have been jealously guarded as *secrets* by the persons connected with the respective harbours; though it is manifest that any observer who chose, might easily put himself in possession of the information. But their secrets were very safe: for the attention of scientific men had scarcely been at all directed to the comparison of theory and observation. It deserves to be recorded, that in this country, Flamsteed (the father of British Astronomy) was one of the first to attempt to obtain something like accurate observations of the times of high water in the Thames. Observations have been regularly kept at Liverpool and some other ports for a number of years. Since the establishment of the London-Docks, in 1804, a regular system of observations has been carried on there. These have been investigated by Mr. Lubbock, who, with the assistance of M. Dessiou, went through a laborious series of computations upon the data thus obtained, and found some remarkable coincidences with the mathematical theory. A full account of these researches appeared in the *Philosophical Transactions* for 1831, Part II. He also compared one year's observations at the East India Docks, made with great accuracy by Captain Eastfield. But one of the most valuable and interesting features of that paper is the collection of such data, as to the time of high water at different parts of the world, as could be collected from different voyagers and other sources, and which are laid down in large charts; thus presenting to the eye a sort of map of the actual course of the tides.

Again, in the *Philosophical Transactions* for 1832, Part II., a further comparison was made of theory with some observations at St. Katherine's Docks, by the same gentleman; and in Part I. for 1833, at Portsmouth, Plymouth, and Sheerness; and in another paper in the same collection for 1834, Part I., this indefatigable and skilful investigator has given a number of highly interesting details on the variations due to the influence of winds, &c., in the Port of London, and has improved the calculations by taking into account the moon's motion in an inclined orbit, which had not before been attended to.

Mr. Whewell, of Cambridge, (whose high mathematical ability, and comprehensive turn of mind, had led him to the most general views of the relations of the different branches of Science,) had, at the same time with Mr. Lubbock, been turning his thoughts to the subject of the Tides. He had forcibly remarked that "the theory of the Tides is now in the state which that of the moon and planets presented about a century ago; and unless considerable exertions be made, it may so continue for many years to come. The tables of the planets have only acquired their present accuracy through the liberal encouragement of learned bodies, and of some of the governments of Europe: nor can tables of the tides adapted to the present state of Science be now constructed, unless a very considerable expense be incurred from the immense labour required."

We have already referred to some of the systems of observations which have been set on foot with such praiseworthy diligence for securing the most accurate information on which to ground these calculations and tables. The zeal of Mr. Whewell has animated a number of other observers; and owing, in a great measure, to the circulation of notices, rules for observing, &c., by that gentleman, and the stimulus and encouragement which has been given to the subject at the meetings of the British Association, we believe that a number of able labourers are now engaged in the work of obtaining data in different places, which will contribute materially to the formation of accurate tables, and enabling us to verify, with some degree of precision, the indications of theory.

In the *Philosophical Transactions*, 1833, Part I., appeared Mr. Whewell's "Essay towards a first Approximation to a Map of Co-tidal Lines." We have already stated that a chart of this kind had been given by Mr. Lubbock. Even in the short interval since that time, however, so much had been done in collecting observations, that Mr. Whewell found himself in a condition to give a far more complete and accurate map of the actual position of those lines running in a variety of irregular curves, yet all preserving a certain degree of symmetry and dependence upon each other across the surface of the ocean, and extending up into the lesser seas, which mark the connected series of points at which it is high water at the same instant of time, as at 12, 1, 2, &c., o'clock; a distinct line being laid down for each hour. The forms which these lines assume, then present the track which is followed, as it were, by the continuous *crest* or *ridge* of the great tide-wave; and it is impossible to inspect Mr. Whewell's chart without having at once conveyed to the mind a far more complete apprehension of the mode in which this striking effect is propagated throughout the seas, than could possibly be done by any verbal description. Nevertheless, the author, with that caution which distinguishes the true philosophic inquirer, has only ventured to call it a first approximation; as, doubtless, future and more extensive observations will introduce many corrections in the details.

From what we said at first, it will be evident that in the varied courses assumed by the different branches of the Tide, there must be great complexity in the smaller seas, broken and interrupted by promontories and islands; and in none more so than in those seas which surround the British islands. To follow out all these particulars, as far as they can be collected from observations in all parts of the world, and to compare them as well as we can with what theory would indicate, is the object of Mr. Whewell in the profound and elaborate paper to which we refer. Of these details, we shall, of course, not attempt to give any idea. The author has, however, followed them up by other researches in the *Philosophical Transactions* for 1834, Part I., entitled "On the Empirical Laws of the Tides in the Port of London;" in which he discusses at length, the present state both of theory and observation.

We have alluded to the stimulus of late given to the prosecution of Tide observations. At the end of his paper, in 1833, Mr. Whewell had suggested, that if simultaneous observations could be made on the heights and times of the tide for an entire fortnight or half lunation, at different points of the coast, the most valuable accession would be made to our knowledge. The stations of the Coast-guard seemed to offer every facility for the purpose. Accordingly, a representation being made to that effect to the superintending officers of that department, it was met with the utmost readiness and promptitude. Under the directions of Captain Bowles, the chief commissioner, and Captain Beaufort, the hydrographer of the Admiralty, a plan was immediately organized, in accordance with which, observations at the same time were made at all the stations along the coast of England, Scotland, and Ireland, uninterruptedly, from June 7th to June 22nd (1834), inclusive. These valuable observations were all reported to a common centre; and the various calculations which then had to be made upon them were in a great measure executed by M. Dessiou, of the Admiralty. By their means, Mr. Whewell has been able to lay down, with increasing accuracy, the course of those branches of the great tide-wave coming from the Atlantic, which divides into several minor branches, and occasions so great a complexity in tracing out the real origin of the Tides at different parts of our coasts. These researches, however, throw much new light on the subject, and have enabled the author to pursue the comparison with theory to a far greater extent than he was able to do before. They are detailed in the first part of the *Philosophical Transactions* for 1835.

But for the still further prosecution of the inquiry, co-operation with foreign states was requisite; and a representation to this effect having been laid before the Lords of the Admiralty by Captain Beaufort, the subject has been taken up by their lordships, and applications to foreign powers have been made for a combined prosecution of such observations in different parts of the world.

At the Dublin meeting of the British Association, Mr. Whewell gave a public address on the subject; in the course of which he dwelt with peculiar emphasis on the ready and liberal spirit of scientific co-operation thus manifested: "In every case," he observed, "these applications were cordially met; and there was not a maritime state in Europe, not one north of the equator, that was not contributing its assistance to this great work. Sweden, Denmark, Russia, Spain, France, Holland, and the United States, had all joined in it. . . . . . By the next meeting, he hoped that the inquiries commenced under these favourable auspices would lead to some valuable results."

We must not omit to add, that in the same address he adverted to an important discovery, closely connected with the theory, which had been announced to the physical section, by Mr. Russell, a zealous and scientific engineer, connected with the canal navigation of Scotland. That gentleman, in the course of a number of valuable trials, on a large scale, with regard to the motion of water in canals, had discovered that the *velocity* with which a wave is propagated, has a certain determinate mathematical relation to the *depth* of the water. Such a law must manifestly be of great value in showing what ought to be the influence of the varying depth of the ocean on the motion of the tide-wave.

THE JUVENILE PHILOSOPHER.

No. I.

MANY of our younger friends are apt to say, "I should very much like to see experiments in Natural Philosophy, and to try them myself; but then it will be necessary to have a variety of curious instruments and expensive apparatus, which are out of my reach:" we shall, therefore, devote an occasional page to showing our young philosopher what a great number of interesting experiments he may perform *with no other apparatus than a few of the most common articles*, which are everywhere at hand, and the exercise of a very little patience and perseverance in learning to follow the directions which we shall give him.

How to make a Prism.—Take two little bits of broken window-glass, and a lump of wax. After having softened and moulded the wax, stick the two bits of glass upon it, so that they meet together at an angle, as represented in the annexed sketch (fig. 1.) Where *w* is the wax, *g* and *g′* the glasses stuck to it. The end view, or section of it (fig. 2), will show the angle *a*, at which they meet. Into the angle thus made by the glasses, *put a drop of water:* it will stay there by what is called "*capillary attraction.*" This drop of water is represented by *d*.

Fig. 1.

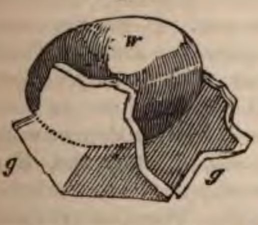
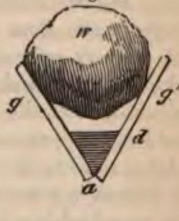


Fig. 2.



Now *to use* the instrument which you have made, you should have a *small hole* in the upper part of the window-shutter, or still better, a narrow horizontal slit, so that you can see the white clouds or sky through it, when you stand at some distance from it in the room. If you cannot manage this, a sheet of pasteboard stuck up in the upper part of the sash with a slit cut in it will do. The slit should be about one-tenth of an inch wide, and an inch or two long, with very even and straight edges. Then holding your prism in your hand, (and for convenience you may stick a small handle on to the wax,) place it close to your eye, and, looking *through* the drop of water, you will see what is called the "spectrum;" that is, a beautiful train of colours,—at one end red, at the other violet, and in the middle yellowish green. To give you a better idea of the position of the whole, and the direction in which to look, we subjoin another figure. Here *e* is the eye of the spectator, *p* is the prism, *h* the hole in the shutter, *s* the spectrum.

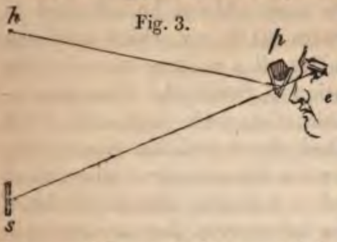


Fig. 3.

Now when you have tried this two or three times, you will get the knack of easily looking in the right direction, and will see the colours very bright and distinct. If you wish to make it more than a mere amusement, and are desirous to learn something from it, write down *what colours* you see, and in *what order*. Observe what takes place if you turn the prism about a little; and write that down also. Particularly (if you can) get some idea of the *length* to which the coloured spectrum extends, compared with its *breadth*. Thus you may, with a little practice, manage to compare it with some fixed object which may be seen by the side of it, by looking past the prism at the same time. This, perhaps, will be difficult: at any rate, bear in mind the general appearance of the whole; and, if you can, draw a sketch of it. This will be particularly instructive, if you next take out the drop of water, and put in its place a drop of some other liquid,—*oil*, for instance. You will now see a spectrum with similar colours; but when you compare your sketch, in this instance, with the former, you will see a great difference in the length to which the colours are drawn out.

And not only will they differ in this respect, but also the whole spectrum, bodily, will be shifted into a different position; viz., lower down from the hole or slit than in the former case. If you use any common oil this will be very evident; but if you procure a little oil of aniseed, or oil of cassia, it will be exhibited in a most striking and peculiar manner. With these substances the spectrum will be stretched out, as it were, into a very great length compared with that formed by the drop of water. You may also try a variety of other fluids. Almost every one, if carefully examined, will offer some peculiarities worthy of notice.

But this method is not confined to fluids. There are even some transparent substances of a more solid nature, which may (with a little dexterity) be used in the way described. For instance, a small piece of gum, or turpentine, may be *pressed* and squeezed into the angle formed by the two glasses, till it forms a small prism, in the same way as the drop of liquid did before. In this way, the properties of a number of different substances may be examined and compared. We suppose all along that the glasses are kept at the *same inclination* to each other. If this be altered, the length of the spectrum will be changed owing to this cause *alone. You may try* the *same drop of water*, and *vary* the inclination of *the glasses.*

RECENT RESEARCHES ON THE FORMATION OF RAIN.

In a climate like ours, the Science of Meteorology, and especially that department of it which relates to *rain*, possesses a peculiar interest. It had been generally understood by philosophers, that the formation of rain was simply due to any sudden condensation of the moisture of the atmosphere at a great height, which so condensed, falls in drops. The air holds always, more or less vapour in solution, in the state of an invisible elastic fluid, but if cooled to a certain degree, this is condensed in the form of *visible* vapour, that is, fog or cloud; and according to various conditions (chiefly electrical), this is either suspended in that state by the mutual repulsion of its particles, or they are attracted together into drops of larger size, and fall by their gravity in showers of greater or less force, according to the quantity, the elevation, and other circumstances.

The attention of observers has lately been called to this subject, chiefly in consequence of some remarkable *anomalies* which were noticed. It was commonly observed that more rain falls in mountainous countries and *elevated* places. This is explained by the circumstance, that the elevations both attract the clouds, and from their greater coldness, promote the deposition. It was therefore a fact apparently quite anomalous, which was noticed in some observations of M. Arago, that at the *top* of the observatory at Paris, the quantity of rain which fell was considerably *less* than on the *ground*. Some observations of the same kind, made by Dr. Heberden, on the roof of Westminster Abbey, compared with a position near the ground, fully confirmed the result. This curious fact, bearing so strongly on the various questions connected with the constitution of the atmosphere, and the formation of its depositions, attracted the notice of several observers in this country, especially Mr. Phillips, the able and scientific secretary of the Yorkshire Philosophical Society, since so worthily appointed Professor of Geology in King's College. This gentleman had turned his attention, among other subjects, to that of meteorology, and found a powerful spirit of co-operation in other individuals, connected with the Yorkshire Society, to assist him in carrying on observations for this express object.

At the first meeting of the British Association, for the advancement of science at York, 1831, the committee, amongst those various other suggestions and recommendations, from which have emanated so many valuable researches and reports, adopted a resolution, "that Mr. Phillips, and Mr. W. Gray, jun., of York, be requested to undertake a series of experiments, on the comparative quantities of rain falling on the top of the great tower of York Minster, and on the ground near its base. The committee have been induced to propose this specific question, in consequence of the local fitness of the situation, and the facilities offered for its solution by the authorities: but it is to be wished that similar experiments should be made elsewhere, that by an extended comparison of observations, light may be thrown on the anomalies which have been observed at Paris, and in other places."

This recommendation was ably and zealously seconded, both by the exertions of the scientific individual who undertook the experiments, and the liberal co-operation of the society on whose premises one portion of them was to be carried on, and of the dignitaries of the cathedral, who afforded the most admirable station for the other part of the operations.

The local advantages afforded for such inquiries at York are considerable. The great valley, or rather plain, which takes a sweeping course through the centre of Yorkshire, from the mouth of the Tees to the estuary of the Humber, and varies in breadth from fifteen to twenty-five miles, presents not a single elevation of any kind, approaching to more than half the height of the central tower of York Minster, which is 200 feet from the ground. Here one of the rain-gauges was mounted on a pole, so as to rise some feet above the battlements of the tower. It was thus in a situation to give really a fair sample of the condition of the free atmosphere. The local circumstances, Mr. Phillips observes, "give an importance to the moderate height of York Minster, which is denied to many loftier buildings in England. From its summit, the course of a passing storm may be well traced from even the distant hills of Richmond; and the deflections occasioned by the attraction of the sides of the vale, the rushing of the air, the sudden fall of temperature, and many other curious phenomena accompanying the precipitation of rain, may be well observed. It is, probably, to the peculiarity of its geographical situation, that we are to attribute the remarkable general regularity of the curves of mean temperature at York."

The other stations were at the Museum of the Yorkshire Philosophical Society; this is situated just out of the town to the West. Its roof is the highest point in the immediate neighbourhood; it stands apart in the society's grounds. A gauge was fixed on the roof, about thirty feet from the ground. In the grounds, at some distance from the building, in the centre of an open lawn, was placed the third gauge, sunk in the ground, having its edge nearly level with the grass. These gauges were of a good yet simple construction; and all the arrangements made with a view to ensuring the accuracy and regularity of the observations were admirable. We speak with confidence, from having ourselves examined the whole, with the advantage of the able explanations of Mr. Phillips, in 1834. To the unabated zeal of that gentleman, science owes much in all its departments; but as our object here is not the praise of an individual, but the exposition of his results, we will only state, that with the most unremitting diligence, aided only occasionally by some friends, especially Mr. W. Gray, jun., of York, he carried on constant observations at the three stations, obtaining carefully, at certain intervals, the amount of rain which had been collected respectively, in the three gauges. The results for an entire year are fully stated in a Report communicated to the British Association, at the meeting of 1833.

These observations establish and confirm in the most undeniable manner, the singular and anomalous result already alluded to, that the *quantity of rain increases*, by some means, during its fall, as it approaches the ground; being greatest at the surface of the ground, less at a small height, and least at a greater height. Mr. Phillips enters into a variety of considerations with the view of accounting for this phenomenon. He commences by endeavouring to deduce some numerical relation, or law, among the observed numbers: these inferences were, indeed, confessedly, but rough approximations in the first instance; they were, however, fully such as authorized the belief of their pointing to some real law of nature. The diminution in the formation of rain in the upper parts of the atmosphere, appeared to be greatest in the cold, and least in the warm months. Its relation to the degree of *dryness* of the air, or the quantity of invisible vapour suspended in it, was a point requiring particular examination: and the degree of diminution was found to hold a very close and simple relation to the dryness.

The results thus obtained, enabled the author to offer an explanation of the phenomenon, possessing a high degree of probability: namely, "that the whole difference in the quantity of rain at different heights above the surface of the neighbouring ground, is caused by the continual augmentation of each drop of rain, from the commencement to the end of its descent, as it traverses successively the humid strata of air at a temperature so much lower than that of the surrounding medium, as to cause the deposition of moisture on its surface." It is *not*, then, an increase in the number of drops, formed at lower points in the air, but *each single drop* increases in size, like a snow-ball, acquiring fresh depositions on its surface, from the moisture of the region through which it passes. The drops descend from a colder region, and bring their temperature with them. We recognise this in the familiar observation, that a shower *cools* the air. The increase takes place not at a uniform, but at an accelerated rate, in approaching the ground; this is a general rule, though not as yet reduced to an accurate law; but it is fully accounted for by the above hypothesis of the mode of increase.

The numerical results, however, yet only furnished general approximations. Desirous, therefore, of obtaining a still increasing accuracy in these important data, Mr. Phillips went through another entire year's set of observations: the account of which was, like the former, communicated to the British Association. This report, presented at the Edinburgh meeting, gave a more decided confirmation of the increase of the quantity of rain as it approaches the ground: the quantities at the three stations being, (roughly,) in the ratio of the numbers, fourteen, nineteen, and twenty-five; the rate of increase being *less* in *warmer* weather. The results were exhibited in tables, and at least, a *general* accordance with a mathematical law was observable throughout.

The subject excited much interest at that meeting, and considerable discussion took place; in the course of which, Mr. L. Howard, (well known for his researches on the meteorology of London, &c.,) stated, that fully admitting the value of Professor Phillips's observations, he differed from him as to the theory. His own opinion was, that the actual *number of drops* increases near the surface, or that the actual deposition from the atmosphere, continues to originate *new rain* at all heights, or at least, at different heights, according to the various conditions of temperature, moisture, &c., but principally, in relation to the *electric* condition of the mass of air or vapour; and though he does not deny that each drop may acquire an accession, yet he thinks it so small as *to be quite insensible.*

Various arguments arose on this objection. We confess for our own part, the circumstance which appears decisive on the question, is the regularity with which the effect follows in *numerical* amount all the *variations* of those causes which should produce it on Professor Phillips's theory; whereas, we see no indication of its following any relation to electric action. That regularity of sequence and constant association in all the variations of one general fact with another, is, we believe, to say the least, the most undeniable *indication* of a real connexion of the two as physical *cause and effect;* the precision of these numerical laws, has since been still more powerfully confirmed.

At the Dublin meeting of the British Association, Professor Phillips presented his report of a *third series* of twelve months' observations by the same method, and conducted at the same positions. The preceding sets of observations having determined the general fact of the increase in the quantity of rain as it approaches the ground, and the dependence of the amount of increase on the mean temperature of the season and humidity of the air, the object of the present inquiry was to trace more closely the *precise law*, or rate of increase, and to compare the indications of experiment with the mathematical formula. Several additional precautions were now employed to guard against various sources of fallacy, which might be imagined likely to affect the results. The calculations founded on these observations, combined with those of preceding years, led to the following general results:—

1. The continual augmentation in size, of every rain-drop as it descends towards the earth through the strata of the atmosphere loaded with vapour. 2. That this augmentation increases *faster* than in the simple increase proportion of the distance from the ground. 3. That the *rate* of this increase varies at different seasons, and in a certain *determinate* relation to the *mean temperature* of the season.

The mathematical laws of these facts are combined in one simple algebraical formula, which is found to represent very accurately the numbers actually observed. Thus Professor Phillips has suggested to us, a sort of philosophical history of a rain-drop; from its birth in the upper regions to its burial in the earth. And it is an instructive history, as it reveals to us the condition of the atmosphere through which the drop has taken its course, under the combined influence of the temperature, the quantity of vapour suspended in the air, the varying currents of the atmosphere, and other circumstances which contribute to the formation of rain.

These observations, however, it must be remarked, apply only to one locality: certainly a very favourable one for such observations. It becomes an extremely interesting inquiry to those who value the promotion of a knowledge of the constitution of the atmosphere, to have similar observations repeated at various other stations. We believe the British Association have organised some system by which such observations may be set on foot in different parts of the kingdom. But it is manifestly, a class of observations in which persons without any profound scientific attainments, with only a little perseverance and accuracy, may do much for the advancement of experimental knowledge. For full details, we refer those interested in the subject, to the several volumes of the Reports of the British Association.

STEAM AND GAS.

Among the physical agents which, by stimulating our curiosity in the examination of their qualities and habits, are wisely appointed to minister to our necessities and our enjoyments, there are none, perhaps, which have exercised the ingenuity or tested the patience of man with happier results than *water* and *coal*.

Steam and Gas furnish decisive and beautiful illustrations of what untiring perseverance is capable of accomplishing. They exhibit some of the elements of nature brought into a state of combination that fits them, if left uncontrolled, to spread terror and death; but when under proper management, we behold them in such a state of subjection, that it may literally be said "a little child may lead them."

So intimately associated are steam and gas with our own age and country, their properties and capabilities occupy so large a portion of public attention, in almost every part of the civilized world, that we think we cannot introduce more interesting subjects for contemplation, than are suggested by some of the modes of employing the one and of preparing the other. All that we intend at present is a few general remarks. Let these be viewed as introductory to more elaborate accounts.

The active properties exhibited by steam, and by which it is so eminently adapted for a motive power in machinery, are due entirely to heat. It is not worth while to attempt a solution of the question, What is heat? because we believe the endeavour would be only a waste of time. Heat may be an elementary substance, existing, in some cases, independently of matter, or it may be merely a condition of matter, and inseparable from it; yet influencing its forms, and producing in it changes, with a degree of certainty, that is equalled only by its extraordinary energies. From what we know of heat, by its effects, we may affirm that it is a highly-refined, an all-pervading, and an irresistible agent. It is known to us only as it is combined with the diversified forms of matter, and we know nothing of matter unassociated with heat. With this ignorance respecting the nature of heat, it seems we must at present be contented.

Steam is water in a very minutely divided state; by which division it is capable of containing, and of carrying along with it, under particular circumstances, to any required distance, a greater quantity of heat than when in its ordinary state as a liquid. But whilst this capacity for heat, as manifested by water in a state of vapour, is one cause of its great utility, it possesses another peculiarity not less important, namely, the facility with which a certain portion of the heat may be separated from the water with which it had been temporarily combined; the steam, by a very simple expedient, instantaneously assuming the liquid form, and the heat as quickly disappearing, and taking up its abode in some other material.

Heat and water, then, are the primary sources of motion in those wonderful combinations of machinery denominated the steam-engine. At no very distant period, it will be our business to show how these powerful agents co-operate in producing those results which, whilst they excite our admiration, should also awaken our gratitude.

Turn we now, for a few moments, to gas, which, like steam, owes its existence to heat. In a general sense, gas implies those substances which retain the aëriform state under ordinary circumstances of temperature and pressure. We shall limit our observations to *coal-gas;* that being the material to which we alluded in a former part of this paper, and which is now so extensively employed as a medium of artificial light in this country, in many of the principal towns on the Continent, and in the United States of America.

We mentioned above, as a valuable property of steam, that a part of the heat it contained could be so easily separated from it; entering into some other material, and leaving the water as it found it, that is, in the liquid state. A property the very reverse of this is possessed by coal-gas. The inflammable and luminous elements which enter into combustion with heat, in the formation of this curious substance, retain so firm a grasp of each other, if we may be allowed the expression, that neither cold nor pressure of any ordinary kind will separate them. Hence it is that gas may be stored and kept ready for use, and transmitted with certainty, both as respects time and quantity, to any distance from the place where it is produced. To those who have never thought much upon this subject, it may appear strange that a part of the heat, and consequently, of the light, emanating from gas, burning several miles distant from the manufactory, is the very same heat and light which, a few hours before, had been produced by the combustion of coal or coke. Such, however, is the fact. The heat arising from the ignited fuel passing through the retort, and combining with certain elements in the coal, constitutes gas. That gas, whether stored for use, or immediately passed into the mains, finds its way, in a little time, to the burner,—it may be in a street-lamp, a shop, or a drawing-room; but wherever it makes its appearance in a state of ignition, it there yields up a part of the heat it received at the manufactory; its elements are transformed, scattered hither and thither, and are thus prepared for new combinations in the economy of the universe.

LEVELLING.

SIMPLE AND EFFECTUAL MODE OF DETECTING ERRORS IN LEVELLING OBSERVATIONS.

In the best modern levelling-staves, as for instance, those for which a Telford medal was awarded to Mr. Gravatt, C.E., last year, by the Institution of Civil Engineers, the observation is at once read off by the surveyor, instead of being reported to him by the assistant; a saving of time, and a diminution of the sources of error, are the consequence. But still, if a surveyor, on the conclusion of his field-work, suspects an error, he has no other means of discovering the place of the error, or removing the suspicion, but recommencing the survey and repeating part, probably the whole, of his observations. A very valuable suggestion has been made by Mr. Henry E. Scott, which, if adopted by a surveyor, would, almost to a certainty, enable him, by merely referring to his field-book, and without the repetition of a single observation, to detect the place of the error, and correct it; or, in case of there being none, to restore his confidence in his observations and final result. Mr. Scott's practice is to have the front side of his levelling-staff graduated from the bottom as usual, and painted in black and white; but, in addition, he graduates the rear-side of his staff, and paints it in *red* and white. This red graduation is in the subdivisions the same as the front one, but the position and numbering of the principal divisions are different: the first principal red division being made at 0·75 ft. from the lower end of the staff, and numbered III.; the next above, IV., and so on. Both sides of the staff are to be read off at each observation; and it is evident, that two very different heights of each observed point will be recorded; that from the red side being constantly 2·25 ft. higher than that from the black or the true one. A difference so wide, that the memory can never act disadvantageously in reading off the quantities on the two sides. An error in the levels can, by this mode of registering, be detected by a single glance; for the surveyor has simply to ascertain the place where any two observations of the same point have not the regulated difference. If no case of this kind occurs, there arises a feeling of confidence in the accuracy of the whole level-survey, which can scarcely be shaken.

EXPERIMENTS ON THE CIRCULATION OF THE BLOOD.

BY PROFESSOR HERING, OF HEIDELBERG.

FROM the very first hour that the circulation of the blood was suggested, to the present day,—during the period of doubt and examination which naturally followed its public announcement; and in spite, as it were, of the searching investigations of modern anatomy and physiology, not the slightest suspicion seems ever to have crept in, that the velocity of the circulation did not depend upon the rate of pulsation. No connexion of cause and effect appeared closer or more legitimate than this. Analogy furnished the most decisive instances to support it, when any were wanting; and the possibility of this connexion being dissolved, appears never to have entered the brain of any observer. That the pulsation of the heart and arteries, and the velocity of the circulatory motion, were at all times exactly proportionate,—that the heart was the agent, and its contraction was the only cause, of the sanguineous circulation; and that it acted upon the blood, as a forcing-pump does upon any other fluid, was the belief and the language of the greater number of physiologists of the past and of the present time. The simple denial of a fact so apparently true, and so generally admitted, if it came from a person of intelligence and veracity, would be startling; and the attempt to reduce it, at once to a "vulgar error," would be considered quixotic and absurd. Professor Hering, either by acute observation, and true philosophic indifference to all theories, however old and extensively received, or by some accidental circumstances, has, however, suspected the truth of the notion; he has proceeded from suspicion to experiment, and has arrived at results which, to say the least, throw great doubt upon, if they do not entirely disprove, this supposed relation between the pulse and the circulation.

made to sympathize with one another, should not seek to trace out still more of the mystery of their union, to know more of its nature and laws, and to unravel its cause.

Man is necessarily, and from the very mode and nature of his existence, a speculative being. And of all subjects of speculation, the changes in the heavens are probably those which first arrested his attention. How earnestly must the master spirits of those days, when the secret of the universe was unknown, have wished and have laboured to account for phenomena which we now so readily explain, by means of our knowledge of the form of the earth: how must the mysterious alternation of day and night, and the march of the seasons, have distracted them, wearied their imagination and perplexed their reasoning.

> Quæ mare compescant causæ; quid temperet annum;
> Stellæ sponte sua, jussæne vagenter et errent;
> Quid premat obscurum lunæ, quid proferat orbem;
> Quid velit et possit rerum concordia discors*.

It was in these words that Horace described the sublime but very unsatisfactory speculations of his friend Grosphus.

The mighty changes in the heavens controlling, as they do, all the phenomena of animal and vegetable life, necessarily couple themselves in the mind with the direct agency of the supernatural world, and thus it was that the astronomy of the ancients, became incorporated with their mythology. The sky was Atlas or Uranus,—it was eternal and unchangeable; the fixed stars were its organs of vision; the planets, of which the controlling power was the sun, rolled eternally, according to their notion, in concentric orbs of crystal around the earth. These planets they called gods, and their path was along the milky way,

> Est via sublimis, cœlo manifesta sereno;
> Lactea nomen habet; candore notabilis ipso.
> Hâc iter est Superis ad magni tecta Tonantis,
> Regalemque domum †.

They represented them by letters in the order of their distances.

Moon.	Mercury.	Venus.	Sun.	Mars.	Jupiter.	Saturn.
Α	Ε	Η	Ι	Ο	Υ	Ω

Saturn, the slowest of the planets, was taken as the symbol, and made the god of time, and, like time, Saturn destroyed his offspring; he took the *wings* of time and his name, Χϱονος (*Chronos*).

Jupiter, the most remarkable of the planets for his splendour, supplanted his father Saturn, occupied the throne of the universe, and became the king of gods.

Mars, of the colour of blood, and placed nearer to the sun, they imagined to be endued with attributes of a warrior, and called him the god of battle.

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* "What causes set bounds to the sea, or vary the returning seasons? Whether the stars move of themselves, or by the order of a higher power? What darkens the face of the moon, or extends her to a full orb? What is the nature and power of those principles of things, which, although always at variance, yet always agree?"

† "There is a way in the exalted plain of heaven, easy to be seen in a clear sky, and which, distinguishable by a remarkable whiteness, is known by the name of the milky way. Along this the road lies open to the courts of the nobler deities, and to the palace of the great Thunderer."—OVID.

Venus, whose clear bright light is sometimes to be seen even through the daylight; at one time precedes the sunrise, and at another follows the twilight, alternately pursuing and pursued by the sun. They believed her to produce the fertilizing dews of the morning and the evening; named her the goddess of fecundity, of beauty, and of love, and adored her under the names of Astarte, Astaroth, &c.

Mercury, the swiftest moving of the planets, was taken as the symbol of speed and lightness; he became the god of motion; and, being ever seen in the immediate neighbourhood of the sun, was designated the messenger of Olympus.

The Sun was adored as the author of light, order, and fecundity; and the Moon, as destined to imbibe this influence from the Sun, in their conjunction, and transmit it to the earth. All the nations of antiquity erected altars to the Sun. In Egypt he was worshipped as Osiris, in Phenicia as Adonis, in Lydia as Athys, &c.

A multitude of divinities were thus frequently worshipped in the same being; a fact not to be wondered at, since the attributes which each nation assigned to their common object of worship, would necessarily partake in the errors of their knowledge of it, and the prejudices which they had attached to it. And thus, until it pleased God to make a direct revelation of his will to mankind, the history of the developement of the religious principle among them, was little other than a history of the wanderings and uncertainties of the human understanding, which, placed in a world it could not comprehend, sought, nevertheless, with unwearied solicitude, to develop the secret of it, which, a spectator of the mysterious and visible prodigy of the universe imagined causes for it, supposed objects, and raised up systems; which, finding one defective, destroyed it to raise another not less faulty on its ruins; which, abhorred the errors that it renounced, misunderstood those which it embraced; repulsed the very truth for which it sought; conjured up chimeras of invisible agents, and dreaming on, without discretion and without happiness, was at length utterly bewildered in a labyrinth of illusions.

How great is the contrast! Since the age in which the heathen mythology had its origin, the religion of mankind has fixed itself upon the sure foundation of a revelation from God, and the human understanding has acquired for itself the master-secret of the universe. The wanderings of the stars on the firmament of the heavens are at length understood. The question

Sponte sua jussæne vagenter et errent?

no longer perplexes us. We find throughout the whole of what appeared to our ancestors the capricious motions of powerful but isolated beings, evidences of one impulse, one will, one design, one Almighty power, originating, sustaining, and controlling the whole. These beings then, to whom, calling them their gods, it was natural that they should attribute a separate, independent, and capricious existence, subject to the indecision, the error, and the feebleness of humanity, appear to us but as the creatures of one sovereign intelligence, bound down in as passive obedience to that intelligence, as the stone that falls from the hand, or the apple that falls from the tree; with no other thought, or will, or power, than that of any particle of dust blown about by the Summer's wind. Thus the whole of the sublime and gorgeous pageantry of the heathen mythology vanishes like the baseless fabric of a dream.

We know that this magnificent phantom retained its shadowy control over the intellect of man, in an age of great literary refinement, of profound knowledge in the philosophy of morals, and of high civilization; and had no revelation interposed, there could be nothing found in the mere literature, ethics, and civilization of *our* day, as distinguished from the literature, ethics, and civilization of theirs, to overthrow it; thus we might still, in respect to these, be what we are, and yet the worshippers of a host of gods: but combine with these the *science* of our times, and the supposition becomes impossible; a single ray penetrating the mystery of the universe is sufficient to dispel the illusion of Polytheism, and instruct us in the knowledge of the one only and true God.

How prodigious has been the progress which the universal mind of man has since made, how wonderful the vantage ground on which *we* stand, when we look forth upon nature; the human intellect now walks to and fro in creation, as with the strength of a giant, the growth of whose stature has been through ages, and who but yet approaches the noontide of his vigour.

The first question which suggests itself to a mind curious to understand the phenomena of the heavens, is probably this—ARE THE SUN, MOON, PLANETS, AND STARS, really as they seem to be, at the same distance from us, and almost within our reach? or are they, as we are told, some of them infinitely more remote from us than others; and the nearest of them distant more than half a million of miles? Our first inquiry shall then be

WHAT IS THE PROBABLE DISTANCE OF THE FIXED STARS?

Are they, as we are told, many millions of miles away from us; so far, indeed, that their light, travelling as it does at the rate of 80,000 leagues in a second, has from the nearest been six or eight years in reaching us? And if it be so, how is this known?

Let us suppose an observer to have travelled about, far and wide, on the earth's surface, and accurately to have observed, as he went on, the appearances of the heavens; he will at once have perceived the stars to be bodies scattered about in that great space, whatever it may be, which contains the earth, and he will have remarked that they do not alter their apparent relative positions, as he moves about on it. Their apparent positions, with regard to the *horizon*, are, indeed, continually altering; but with regard to one another, he finds them always the same. This will appear to him very extraordinary, when he considers that the various objects around him on the earth's surface, are continually subject to apparent changes of relative position, as he moves about from one place to another. Thus for instance—let him be sailing along the sea-coast at night, and let him observe two lights upon projections of the shore. At one instant, when he is in the line joining the lights, they will appear to him to coincide, blending momentarily into one light; as he proceeds, they will appear to separate, or, in the nautical phrase, they will *open;* and this opening of the lights will continue, until they have at length acquired a certain maximum apparent distance. They will then appear to approach one another; and, as he finally leaves them behind him, they will go through all the same circumstances of apparent motion as attended his approach to them. If the lights be sufficiently remote, all these changes in their apparent distance from one another, will be referred to, and apparently take place upon, the circular margin of the horizon. They will seem like two beads of light moving towards one another on the *circumference* of that circle; coinciding, then receding, and again approximating to one another. These apparent motions are called parallactic*.

Analogous changes of *bearing* may be observed in objects situated at different distances from us, in the day time.

Now, why are there not changes of apparent relative position like these among the stars?

A slight consideration will show him that this can only be accounted for by supposing the distance of the stars to be exceedingly *great*, as compared with any distance through which he can himself move. He can prove demonstratively that the parallactic motion arising from any given change in his point of view, is necessarily less as his distance is greater; and that when that distance is extremely *great* in comparison, and *then* only, the parallactic change in the position of the object is insensible.

Let a man look through his window at any two stationary objects without,—two chimneys for instance; if these be no great distance from him, he will perceive, that by moving his position ever so slightly, their apparent angular distances from one another will be changed, and he may, indeed, readily so far change them as to cause one to appear behind the other. Let him now look at two other objects more remote than these, he will find that the same motion of his point of view will not produce the same variation in their relative positions: and if the objects be *very* distant, the variation which he can thus produce will be imperceptible. Were he, however, to use an *instrument*, such as are every day constructed for measuring the angular distances of distant objects, there are scarcely any two within the reach of his vision, which would not appear under different angles, when viewed from different parts of his room.

It is upon this principle, as has been observed by Sir J. Herschel, that in Alpine regions visited for the first time, we are surprised and confounded at the little progress we appear to make by a considerable change of place. An hour's walk, for instance, produces but small apparent change in the relative situations of the vast and distant masses by which we are surrounded. Whether we walk round a circle of a hundred yards in diameter, or merely turn ourselves round upon its centre, the distant panorama presents almost exactly the same aspect—we hardly seem to have changed our point of view.

On the whole, then, since, when we pass from one point on the earth's surface to that which is even the most remote from it, we perceive no change in the apparent relative positions of the stars of that kind which has been called parallactic; it follows, with the most certain evidence, that these stars are immensely distant from us.

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* *Parallax*, the angle formed by two different lines of view drawn towards one and the same object. Suppose a point is seen from the two ends of a straight line, the two lines of view towards that point form, with the first line, a triangle, whose angle at the point seen is the parallax, or parallactic angle. *Annual parallax*—the angle formed by two lines from the ends of one of the diameters of the earth's orbit to a fixed star, which angle, on account of the immense distance of the fixed star, is too small to be observed.

But a still more accurate notion of the effect of parallactic change may be obtained as follows: let a circle be measured only a few yards in diameter, and an observer walk round it, measuring, with an instrument contrived for that purpose, the angular distances of two objects, only just visible on the edge of the horizon; he will obtain in every different position, a different measurement; and instruments have been made of such nicety, that different positions on such a circle would give differences in the angles observed, even when the distances of the objects were at least 100,000 times the diameter of the circle. Now instruments of this kind, and of the most perfect workmanship, have been employed to observe the angular distances of the stars from points differently situated on a great circle of the earth, and no parallax has ever been traced*. It follows, therefore, *demonstrably*, that the distance of the stars is more than 100,000 times the diameter of the earth. Now the earth's greater diameter is 7925 miles. Imagine, then, these 7925 miles taken 100,000 times, and a great sphere described, having that line for its diameter, and the earth for its centre; we are certain that the region of the fixed stars is without that sphere.

Although the fixed stars are thus observed to have no parallactic motion, yet the sun, the moon, and the planets have. These we may conclude then, with equal certainty, to lie within that imaginary sphere of which we have spoken; and their distance from us to be less than 7952 miles taken 50,000 times.

The fixed stars, then, belong to a region greatly more remote than that of the sun, moon, and planets. Now what are these dwellers in the infinity of space? are they material existences, or are they bright spiritual agencies, free of the trammels by which each particle of the system around us is bound eternally to every other particle—free of the laws by which motion, in all the variety of its forms, is coupled here with motion? If not, what are the laws which govern them? are they the same with those infixed upon the component material of our nether world, or are they other than these? Do they constitute a portion of the *same* infinite and immutable sequence of passive being, or of some other? Did the *same* Hand strew them upon the face of the heavens; are the laws by which they are governed emanations of the *same* Intelligence?

Astronomy answers these questions unhesitatingly. The ancients believed them to be the eyes of Uranus, the spirit of the universe. Astrologers of the middle ages attributed to them superhuman powers, and an active and capricious agency in the affairs of men. The astronomer declares them to be of brute matter; as passive and inert as that beneath our feet—as the clod or the pebble which we kick from our path,—matter subject to the same law by which the stone, thrown into the air, is made to deflect itself in a curve to the ground; the moon to gyrate about the earth, and the earth and planets about the sun. Thus the astronomer, as with a chain, binds the whole of visible existence by one great and immutable law to the throne of one great Intelligence; and, with the finger of demonstration, inscribes the name of one God on the altar of the universe.

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* Still further experiments have, for sometime, been preparing, or are in contemplation, at the Royal Observatory, Greenwich.

Now let us consider, shortly, how this continuity of matter, and this universality of the laws which govern it is established.

The stars, called *fixed*, are not, in reality, fixed; they all move in a certain given direction through space, with a motion which will eventually mingle the constellations. The 61st star, for instance, of the constellation called the Swan, is ascertained to move in a straight line over five-seconds of the heavens annually; and, by the observations of Messrs. Mathieu and Arago, it appears that this star cannot certainly be distant from us less than 412,000 times the diameter of the earth's orbit; that is 412,000 times 39,000,000 of leagues. Now, if we suppose it to be at this distance, its motion through the heavens could not appear to us to be five seconds annually, unless it actually travelled through a space of 8 millions of millions of leagues in that time. This is one of those stars which, but as yesterday, were called *fixed* stars. But a motion of the whole host of heaven, as with one common consent, and in one common direction through interminable space, is not the only motion which the astronomer sees amongst them: there are stars which revolve round one another.

When first telescopes of any considerable power came to be made, it was discovered that certain stars which appeared to the naked eye to be single, when seen through these telescopes, resolved themselves into two, and these were called double stars. Stars of this kind have since been ascertained to be very numerous; and groups have been found not only of two but of three and four stars. Of 120,000 stars examined, to ascertain whether they were multiple stars or not, 3057, or about one in forty, were found to be so; and were our telescopes sufficiently good, it is possible, if not probable, that all the stars which appear to us single would resolve themselves into systems or groups.

Of the stars of which these groups are composed, one is always found greatly to exceed the rest in splendour. It was imagined at first, that this difference of brilliancy resulted from a great difference of distance; and did this difference of distance exist, it would offer a means of ascertaining the parallax, and therefore the actual distance of the whole group. Under this impression, Sir William Herschel undertook a series of observations at Slough, hoping to discover a parallactic motion in the stars; and, as continually happens, if people would but acknowledge it, in seeking for one thing he found another.

He discovered that, almost in every case, groups of multiple stars, of unequal magnitude, were not, as had hitherto been supposed, bodies isolated and independent of one another, placed by chance, so that lines drawn from them to the eye nearly coincided, but that they are systems, of which the greatest of the group is the central and controlling mass, round which the lesser stars of the group continually revolved, as do the planets of our system round the central sun.

In a group of two, for instance, the lesser star will sometimes be seen to the east, at another in the west, or to the north or south of the greater star. Here, then, is a direct verification of those speculations on the plurality of worlds, which have so long occupied the attention of men of inquisitive minds. Here are systems peopling the whole of space, which revolve round one another as do the bodies of that system of which the planet on which we live forms a part. But is this analogy *complete?* There are certain peculiar laws which govern the motions of the planets which compose our system, indicating, demonstratively, the nature of the force by which they are impelled towards their common centre. These laws are very remarkable; they are called Kepler's laws: one of them is, an imaginary line being drawn from any planet of our system to the sun, although each such planet moves not in a circle but in an ellipse or oval, and not with the same constant velocity, but with a velocity continually varying; yet the space over which the line spoken of sweeps, in a given time, say a week, in any one portion, is the same as that over which it sweeps in any other: this is called the law of the equal description of areas. Another of Kepler's laws is this: The larger axis of the ellipse being called its axis major, the periodic times of the different planets of our system are to one another in the ratio of the square roots of the cubes of these axes majores. These two laws depend upon the fact, that the planets and sun attract and are attracted by one another.

If then we find among these distant groups or systems of suns, the same equal description of areas, and the same ratio of periodic times, we conclude that the stars of each system attract one another, and that the forces by which they are attracted vary inversely as the squares of the distances, and are therefore similar to gravity; and, lastly, that motion is there governed by the same laws as here. Now we do find this to be the case. The motions of double stars have been very accurately observed, among others, by Sir J. Herschel, and he has ascertained that their motion is subject to these laws. He has accurately determined the periodic times, the axes majores, and eccentricities of eight of them; and in every respect does he find the relations which exist between the planetary motions to obtain among the bodies which compose these far remote systems. What then, is the conclusion, but that all these multiplied and isolated systems which people space, and of which the universe is the aggregate, are subject to the same laws of motion and of force as obtain here. Thus the laws of gravitation and motion, which Newton showed to embrace at once the fall of bodies at the earth's surface, and the phenomena of our planetary system, must be extended to the region of the fixed stars. With us, all matter is crowded with life, every interstice in it is but the habitat of some organized living agent; or the space wherein some form of vegetable life develops itself. Now, the matter of the planetary bodies is analogous to ours in every other respect, why not in *this* too?—that it is the appointed dwelling-place of organized living beings. And if of the subordinate classes of these, why not of *intelligent* living beings? Surely, in the absence of all evidence of an opposite state of things, we are bound to conclude, by far the most probable supposition to be, that our planet, which is in every other respect a sample of the other bodies of our system, resembles them in this also; they, as well as ourselves, have their day and night, their summer and their winter; why, as with us, should not these changes be coupled with the phenomena of animal and vegetable life? What a prodigious field of speculation is thus opened to our view. Mercury, for instance, completes his year in about one quarter of ours, and he receives about seven times as much heat from the sun. What then is the vegetation, and what the class of living beings, suited to this rapid change of seasons, and glowing temperature? Jupiter's year is nearly twelve of ours, and each of his seasons is thus three years in length; what gigantic vegetation is that which goes through this toilsome period of change. His day is about ten hours long: what developement of animal life is that whose periods of repose come more than twice as frequently as our own? Four bright moons illumine the short night of this planet: why is this short period of repose brightened almost into daylight?

But if it be by far the most probable of these hypotheses, to suppose that the planets of our own system, because of the analogy they bear in other respects to our own planet, display with it the wonders of animal and vegetable creation; then must the planetary systems, which unquestionably surround the stars, too, having a direct analogy to those of our system, be admitted to be, like them, but the means, but the agents, in the dissemination of life through all space; thus all the boundless fields through which the stars of heaven take their course, are peopled with beings, who bow before God in speechless thanksgiving for the enjoyment of the blessings of life; or whose privilege it is to offer to him the incense of reason and of the understanding.

The subject is overwhelming in its sublimity; let us, however, yet pursue it one step further. One or both of the stars, composing each multiple system, shine not with white, but with coloured light; and their colours are for the most part different; every variety of colour is *found*, but the prevailing colours are blue, and green, and yellow; these facts have been examined with great care, and may be considered as established. Now, what are the phenomena to which these different colours of their double or triple groups of suns must give to the systems of planets by which each group is, beyond all doubt, accompanied; three different coloured suns at once, or alternately, traversing their skies,—days of green light, of blue light, of yellow light succeeding each other, or blending their hues,—seasons in which these colours alternately prevail. These are speculations in which the imagination exhausts itself.

In the course of this introduction, I have on more than one occasion spoken of the truths of Natural Science, with a direct reference to the wisdom, the power, and the goodness of the Author of Nature. I have done this advisedly, and from a belief, that were it not an impiety to discuss the manifestations of infinite wisdom and goodness in created things, otherwise than with sentiments of gratitude to the Creator, and deep humility before him, it could at *best* be considered but as an *affectation*, or a *folly*. It is impossible to consider that instruction complete, which having for its object the developement of the relation of cause and effect in those portions of the sequence of natural things which lie within the scope of sensation, does not point out their dependence upon that First Cause which is beyond it. The study of Natural Philosophy and Natural Theology, if rightly pursued, are one; and true Science but a perpetual worship of God in the firmament of his power.

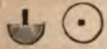
ON THE MANUFACTURE OF POLISHED STEEL STUDS AND BEADS.

By Thomas Gill.

These studs and beads are employed in great quantities by the steel-workers, by the makers of steel dress sword-hilts, and in what the French term *bijouterie en acier*, or steel jewellery, of which we shall treat hereafter; they, therefore, form manufactures of considerable importance, but we believe, they have not hitherto been described by any writer.

The best Steel Studs are made out of decarbonated cast-steel; the commoner kinds are cut out of sheet-iron, of a proper thickness, and are formed into small round or oval pieces, by beds and punches, in the screw-press, in the usual and well-known manner, and which, therefore, need not be described here. Each piece has, afterwards, a hole made partly through the middle of it, at its back, by means of a pointed steel-punch and hammer, to receive a stem of pointed iron-wire, which is driven hard into it, to retain it in its place, until it is afterwards secured more firmly by soldering. The appearance of them in this state will be similar to those shown in fig. 1, which is an edge-view, or section, and a plan of one of them. A number of these studs, thus prepared, are then enclosed in wetted brown or rope-paper, together with bits of brass, as solder, and a little borax, as a flux, and the whole wrapped up into a cylindrical shape; this is then covered with a crust of clay, leaving, however, a small hole at one end of it open, for a purpose to be hereafter described. It is then placed in a forge fire, gradually and carefully heated by blowing, and, at the same time, turned round a little, from time to time, until a white fume or vapour is seen to issue from the hole previously made for that purpose; this indicates that the brass is fused, the zinc becoming volatilized in its usual form, and thus escaping: the mass is then to be instantly withdrawn from the fire, and to be rolled backwards and forwards upon the ground, so as to diffuse the solder uniformly amongst the studs, whilst cooling. And here, it may be remarked, that the zinc in the brass, which rendered it more fusible, becoming thus volatilized, leaves behind it chiefly the copper with which it was combined to make brass; and it is well known, that copper forms an intimate and close union with iron, when thus heated in contact with it, and oxygen nearly excluded. When cold, the crust of clay is broken off the mass; and the studs will be found to have their stems of wire firmly soldered to them, and ready to undergo the succeeding process, viz., that of being brought to a proper shape by filing them.

Fig. 1.



The next process is that of *case-hardening* them; but, as it is requisite that their stems should remain *soft* afterwards, in order to admit of being screwed or riveted in use, so it is necessary to prevent the action of the case-hardening materials upon them; this is effected by enclosing each stem in a slight coat of clay, and thus cutting off all access of the carbonaceous materials to them.

Animal charcoal is the material usually employed in case-hardening, by the steel-workers; this is frequently procured from the ammonia-works, after the distillation of bones, for the production of the different matters to be obtained therefrom. Charcoal is ground to a coarse powder, and put into a sheet-iron or a cast-iron box; a layer of it being spread over the bottom of the box, a number of the studs are then dispersed, at equal distances apart, over that layer of charcoal; another layer of charcoal is then spread over them, and this, in its turn, receives another deposit of the studs; and so on, *stratum super stratum*, until the box is nearly filled, the uppermost layer being composed of charcoal. The box thus filled, or several of them, must then be placed in an open fire-place, filled with pit-coal, and remain exposed to a red-heat, for a sufficient time, to cause the case-hardening effect to take place, and which will depend upon the size of the articles exposed to its action, or to the depth or thickness to which it is intended to carry the hardening process; it being very desirable, frequently, to limit its action to the exterior surfaces of the articles, leaving their interior still merely soft iron, and this, in order to combine strength or toughness with great hardness, in the delicate, small, and frequently thin articles, made in steel-jewellery. When the case-hardening effect is thought to be sufficiently produced, the whole contents of the box, charcoal and all, are thrown into water, or, which is better, into water the surface of which is covered with a layer of oil, two or three inches in thickness, and which is thought to prevent the liability of the articles cracking in hardening them.

Burnt leather is, by some, preferred to the charcoal of bones, for case-hardening. In order to prepare this, old shoes, or other scraps of leather, are collected, and these are burnt or scorched by being laid upon a fire of pit-coal, made in some open place, away from houses, on account of the ill-smell produced in the burning; the scorching is to be continued until the leather is sufficiently friable to be capable of beating into powder when become cold.

Decarbonated cast-steel is preferred to iron, for delicate works in steel-jewellery, where it is wished to avoid all appearance of flaws or veins in the articles made of it. This process is performed by enclosing the slips, or sheets of cast-steel, in iron boxes, filled with *rusty iron-filings*, and which are greedy of carbon, and deprive the cast-steel of it; thereby reducing it to the state of *the softest and purest iron*, when heated to redness during several days and nights, according to the thickness of the steel; thus treated, it will not harden like steel, by merely heating and cooling, but, after forming the articles of it, they must be case-hardened, as above described.

The cutting and polishing facets, on steel studs, is performed in a nearly similar manner to those upon steel-beads; and we shall, therefore, postpone the description thereof, until we have treated of the manufacture of these latter articles.

Steel Beads, if very small, are cut out of thick sheet-iron, or decarbonated cast-steel, by means of beds and punches, in the screw-press, as in the making of steel studs, but have holes perforated entirely through them. If, however, they are of larger sizes, then they require a different treatment; being formed hollow, out of decarbonated cast-steel, in the following very ingenious manner:—The steel, being cut into circular pieces, by means of beds and punches, in the screw-press, is next to be dished, or made concave, in a pair of dies, fitted concave and convex to each other, in a screw-press, until they have rims turned up around them, at an obtuse angle; these, after being well softened by annealing, are then subjected to the action of another pair of dies, by which their rims are turned up still more; they are then again annealed, and pressed between another pair of dies, which brings their rims into nearly a cylindrical shape; and then, after again annealing, they are pressed by the action of other dies into a perfectly cylindrical form. The next process is, by means of a circular bed and punch, to cut out the entire flat piece of metal, and thus to leave a cylindrical ring only, like a ferrule; this is then placed, after being again well annealed, in a pair of concave dies in the screw-press, in an upright position; and the effect of the dies upon it is, to contract each end of the cylindrical ring a little inwardly; another pair of dies succeeds to these, which still more contract the ends, —and again another pair, and so on, observing to anneal them well between each operation; until, at length, a globular, or oval hollow bead is thus made, having merely small holes in its ends, which are necessary to its use. And thus, and without any joint or seam, is a regularly shaped hollow body formed out of a plate or sheet of steel!

The facets are formed upon the surfaces of the steel studs and beads, either by filing them whilst in the soft state, and in which mode the more expensive kinds are prepared,—as well, also, as others; or, after they are hardened, by grinding upon flat pewter laps, with the assistance of coarse emery and water, in which way the more numerous and cheaper kinds are cut. Their appearance will either be similar to that represented in fig. 2, which are round and oval studs, viewed in plan and edgeways; or like those shown in fig. 3, which are hollow beads, having two holes in each and a wire passed through them. The filed studs and beads being case-hardened, as well, also, as those which were cut after hardening them, are now in the state in which the marks left in them by the file, and those caused by the coarse emery, must be removed by the application of finer sifted, or washed emery, either mixed with water or oil; in the former case, by the use of hard flat brushes, continually rubbed over them backwards and forwards, when cemented upon a broad and extended flat surface; or in the latter employment of oil and emery, by holding them against cylindrical brushes, mounted upon square spindles, conically pointed at their ends, and turned in the lathe, either by the foot of the workman,—the long-wheel, as it is termed,—or in mills, on a large scale, by the powers of water or steam. When this process has been performed for a sufficient length of time, or until the coarse file or emery marks are removed, then a still finer kind of washed or flour emery must be employed, mixed with oil, and applied upon an entirely distinct brush or brushes from those used in the last operation; and this process must also be continued until the finer marks, left by the last emery employed, are likewise in their turn completely obliterated; and the articles will then be fitted to receive their ultimate black polish and lustre.

Fig. 2.

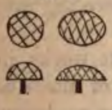
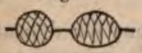


Fig. 3.



Instead of mounting the beads upon cement-blocks, they may be *strung upon wires*, when applying emery and oil to them; and thus save *the trouble of repeatedly* mounting, and again remounting them on the cement-block, in a different position to the former ones, in order to receive the effect of the emery over their entire surfaces.

The last finish, or polishing, can only be properly given to the steel studs or beads by employing putty, or the combined oxides of lead and tin, finely ground,—and either with water, or, which is better, with a mixture of alcohol and water, or proof-spirit, applied upon the soft skin of the palms and inner sides of the fingers of the hands of women! Nothing equal to this process having hitherto been discovered in practice, to give the steel its black polish or lustre.

Having thus generally described the processes employed in the manufacture of polished steel studs and beads, we may here remark, by the way, that the very same method of soldering with brass, employed in fixing the wire stems in the studs, is likewise used by the locksmith, in soldering the delicate wards of his locks; and also by the vice-maker, who thus firmly combines the threads of the hollow screws in his vices with their surrounding boxes, and their other adjoining parts, instead of cutting them, by means of taps, out of the solid metal in the ordinary way; and yet so firmly are they thus united, that an instance of the thread of the screw stripping or quitting its place in the box, by the utmost power applied in using the vice, is a very rare occurrence, and, indeed, scarcely to be met with!

[To be continued.]

JOINT-STOCK COMPANIES OF 1835.

We have extracted a list of the projects which were laid before the public in 1835, and recommended by their several patrons, as eligible means of investing capital. If the list were quite complete, it is supposed the amount of capital proposed to be raised, would equal that of the too-celebrated year of speculation, 1825. Whatever may be the fate of these schemes, it is impossible not to regard without deep interest, the enormous sum of energy, talent, skill, and money, which even the steps necessary for their mere announcement, have put into motion.

Banks.

	Capital.
Agricultural and Commercial Bank of Ireland	£1,000,000
Bank of South Africa	150,000
	£1,150,000

Steam.

British and American Intercourse Company (sea part)	£756,000
British and American Steam Navigation Company	500,000
British and Foreign Steam Navigation Company	200,000
Canterbury, Dover, and London Steam Packet Company	15,000
Equitable Steam Packet Company	120,000
India and London Steam Navigation Company	120,000
Leghorn Steam Navigation Company	28,000
Mediterranean and Levant Steam Packet Company	100,000
Patent Paddle-Wheels Steam Towing Company	30,000
Steam Carriages on Turnpike Roads	20,000
	£1,889,000

	Capital.
Gas.	
European Gas Company	£ 200,000
Greenwich and London Railway Gas Company	20,000
Marylebone Gaslight and Coke Company	75,000
	£ 295,000
Mines.	
Baldhu and Wheal Tregothnau (Tin and Copper)	£ 15,000
Bissoe-bridge (Tin and Copper)	20,000
Candonga	200,000
Combmartin (Lead, Silver, and Copper)	30,000
Chilton Coal Company	50,000
Copiapo (Copper and Silver)	200,000
Carn Grey (Tin)	2,500
East Cornwall (Silver)	50,000
Enterprise Mining Company	20,000
Equitable Mexican Association (Gold)	50,000
Hayle Consols (Copper)	30,000
Kelleweris (Copper)	60,000
Kerrow (Tin)	10,000
New South Hooe	20,000
New Crinis (Tin and Copper)	20,000
New Granada (Silver)	20,000
North Cornwall (Silver, Lead, and Tin)	40,000
Polbreen (Tin and Copper)	30,000
Perran Consols	30,000
Pike Silver Mining Company	112,500
Redruth (Tin and Copper)	100,000
Roche Rock (Tin)	30,000
Royal Copper Mines of Cobre	480,000
Relistian Mining Company	30,000
St. Hilary (Copper)	20,000
St. Geny's (Copper)	19,200
South Polgooth (Tin and Copper)	20,000
Sierra Mining Company (Gold and Silver)	120,000
Treleigh Consolidated (Copper)	25,000
Towan Consolidated (Tin and Copper)	30,000
Terra Putina (Gold)	500,000
Tavistock (Copper)	25,000
Trevorgus (Silver, Copper, and Lead)	30,000
Towedteague (Tin)	25,000
West Tresaveau (Tin and Copper)	60,000
Wendron Royal (Tin)	16,000
West Cork Mining Company	220,000
Wheal Brothers (Copper, Tin, Lead, and Silver)	110,000
Wheal Gilbert (Tin and Copper)	15,000
Wrexham Iron and Coal Company	60,000
Wheals Harmony and Montague (Copper and Tin)	50,000
	£ 3,006,200
Railways.	
Altona, Hamburgh, and Lubeck	£ 300,000
Birmingham and Gloucester	750,000
Bristol and Exeter	1,500,000
Birmingham, Bristol, and Thames Junction	150,000
Brighton and London (Palmer's)	2,100,000
Brighton and London (Gibb's)	900,000
Brighton and London (Stevenson's)	1,000,000
Brighton and London (Cundy's)	700,000
British and American Intercourse (land part)	1,244,000
Blackwall and London	400,000
Blackwall Commercial	600,000
Calcutta and Saugor	500,000
Croydon and London	140,000
Dover and London	1,000,000
Eastern Counties	1,500,000

	Capital.
Gravesend and London	600,000
Great Western	3,000,000
Grand Atlantic	3,000,000
Grand Surrey Canal and Junction	600,000
Great Northern	3,000,000
Grand Northern	4,000,000
Hull and Selby	270,000
La Loire	140,000
Llanelly	200,000
London Grand Junction	600,000
National Pneumatic	200,000
North Midland	1,250,000
North of England	1,000,000
Preston and Wyre	130,000
South Eastern	1,400,000
Southampton	1,000,000
South Durham	150,000
South-West Durham Junction	50,000
Southend and Hole Haven	300,000
Tower of London	1,000,000
Thames Haven	450,000
Windsor and London	300,000
	£ 35,424,000

Miscellaneous.

Anti Dry Rot Company	£ 250,000
Bognor Improvement Company	200,000
British Agricultural Loan Company	2,100,000
Cornwall Royal Tin Smelting Company	100,000
Deptford Pier and Improvement Company	50,000
Danube and Mayne Canal Company	833,000
Equitable Discount Society	100,000
Equitable Society	210,000
Equitable Reversionary Interest Society	300,000
Eastern Metropolitan, Surrey, Kent, and Sussex Society	150,000
Gravesend River Thames Floating Bath Company	20,000
Hastings Improvement Company	200,000
Imperial Anglo-Brazilian Canal, Road, Bridge, and Land Improvement Company	500,000
London Reversionary Interest Society	400,000
Licensed Victuallers' Fire and Life Insurance	150,000
Mexican and South American Company	100,000
Metropolis Pure Soft Spring Water Company	300,000
National Provident Institution	
Norwood Park Estate	20,000
Pennsylvania Coal, Land, and Timber Company	135,000
Prospective Endowment Association	1,000,000
Patent White Lead Company	100,000
Rio De Anori Gold Stream-works Company	25,000
Shetland Fishery Association	100,000
South London Market Company	250,000
South of England Reversionary Interest Association	50,000
South Australian Company	500,000
United Investment Company	50,000
	£ 8,193,000

Summary.

Banks	£1,150,000
Steam	1,889,000
Gas	295,000
Mines	3,006,200
Railways	35,424,000
Miscellaneous	8,193,000
Total	£ 49,957,200

DESCRIPTION OF MR. PERKINS' NEW STEAM-BOILER.

AS COMMUNICATED BY HIMSELF.

THE vast extension of the manufactures of Great Britain, and the facilities of communication, with other advantages, arising from the application of steam, cannot fail to render a description of any further improvements in that powerful agent interesting to our readers.

The following are the advantages which result from a new modification of the circulating steam patent, granted to me in 1832:—

1. Absolute removal of all the danger from explosion.
2. Great economy in fuel.
3. Much reduction of boiler-room, as well as of weight.
4. Not one third of the water in the boiler now used, being necessary.
5. There being no possibility of any deposit of foreign matter in the generators.
6. No furring-up of the boiler, as all the deposit will of itself collect in a place provided for it, and be blown off at will.
7. The generators always being kept at the evaporating point.
8. The impossibility of burning any part of the boiler or generators by the most intense heat.
9. The boiler and generators not being in the least injured by expansion and contraction, owing to the peculiar arrangement of the tubes or generators.
10. The perfect and simple method of separating the steam from the water and foreign matter.
11. The getting up of the steam in less than half the time hitherto required.
12. The simplicity of the construction of the boiler, and the ready means of repair.
13. The power introduced of using Anthracite coal with good effect.

The above facts can be demonstrated not only theoretically but practically. An operating model of this boiler may be seen daily at present (afterwards occasionally), at the Gallery of Practical Science, Adelaide-Street, between the hours of twelve and two.

Explanation of the first-mentioned Advantage.

THE great drawback upon the important invention of steam-navigation has been the disastrous effects caused by the explosion of steam-boilers. The great importance of a perfect remedy will readily be admitted. The many experiments which I have made within the last ten years, go to prove that if the steam be generated in tubular boilers, no danger can result from explosion; but there are many almost insurmountable objections to tubular boilers as hitherto constructed, particularly for steam-navigation. The boiler now about to be described, possesses apparently all the properties required. To show the reason why this boiler is free from *explosions,* the causes (of which there are at least three) must be described.

The *first* and most common cause is from the pressure of common *steam.* *What is meant* by common or pure steam, is such as has not been suddenly elevated, or such as is not compounded with an explosive mixture, by the improper management of the boiler.

This first kind of explosion is harmless, as the boiler simply rends or gives way in the weakest place, caused from wear, or some defective spot. The *second*, cause of explosion which I some years since accidentally discovered and published, (and which explanation has since been experimentally proved to be correct, by the celebrated French philosopher, M. Arago,) arises from the water getting too low in the boiler. The fire then impinging on that part of the boiler which is above the water, causes the heat to be taken up by the steam, which rises by its superior levity to the top of the boiler, causing it sometimes to become red-hot, and so elevating the steam to a much higher temperature than its pressure would indicate. Now, when the boiler is in this state, and the safety-valve suddenly raised, the water will be relieved from the steam pressure, and rush up amongst the surcharged steam which thus receives its proper dose of water; at the same time, that part of the boiler which has been raised in temperature, giving off its heat to the water so elevated, steam is generated in an instant, of such force as no boiler hitherto made can resist. This kind of explosion has of late years been very frequent and disastrous, particularly in America.

The *third** and less frequent kind, although most terrific, is undoubtedly caused by an explosive mixture having been formed in the boiler. It has long been known that hydrogen is often liberated, by the boiler being overheated by improper stoking, as well as not being properly supplied with water; but simple hydrogen cannot explode,—and where it could get its atmospheric air, which is absolutely necessary to form the explosive mixture, it has been difficult to understand. We have only, however, to look at an air-drawing feed-pump, and the source will be readily seen. It is frequently the case that the feed-pump draws air as well as water, arising from its unsoundness, &c. The more air the pump† draws, the less water is forced into the boiler; of course the boiler is more and more exposed to the fire, and the heated parts of the boiler become oxydized, and rapidly liberate hydrogen; and as sufficient air has been pumped into the boiler to form the mixture, it will be ignited by an

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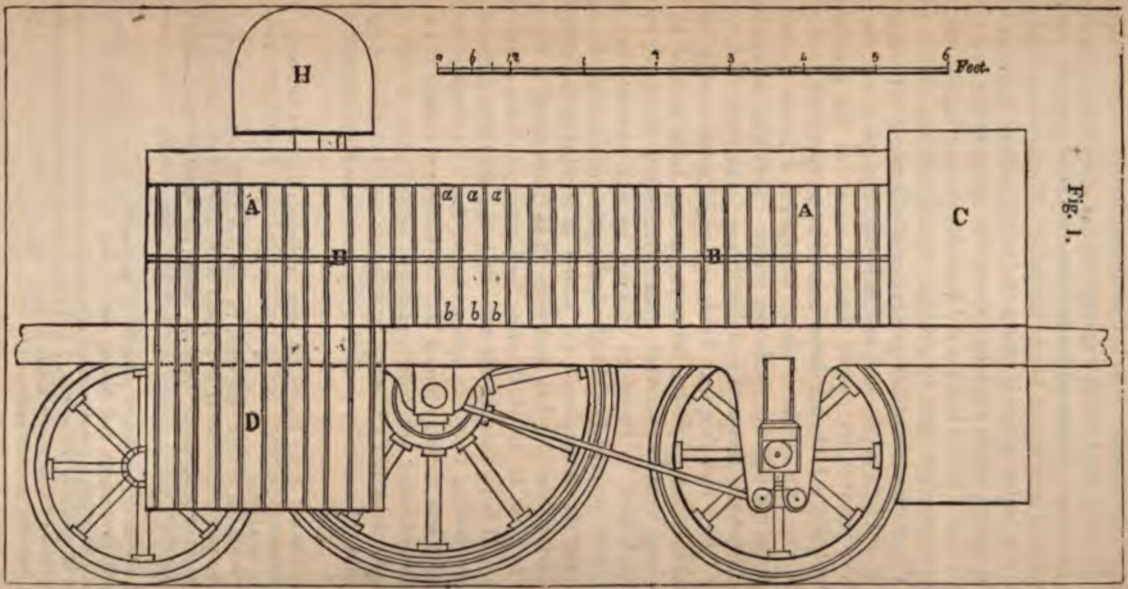
* This theory has not to my knowledge been published; and until recently, I did not see how the atmospheric air could find its way into the boiler, so essentially necessary to form the explosive mixture.

This kind of explosion cannot take place in the new boiler, since no hydrogen is formed in it; for no part of the boiler is exposed to the fire but the bottom, which is certain to be kept at a temperature quite as low as the water in the boiler, which surrounds the generators, by the dashing down of the water outside of the circulating tubes.

Having had about twelve years' practice in generating high steam, from 1500 pounds to the inch downwards, and having established the fact, that no dangerous result has occurred, although a great number of explosions have happened; and having at length removed all practical difficulties, I feel warranted in undertaking to guarantee to the public a system of generating steam of any required power, not only with increased economy, but with perfect safety.

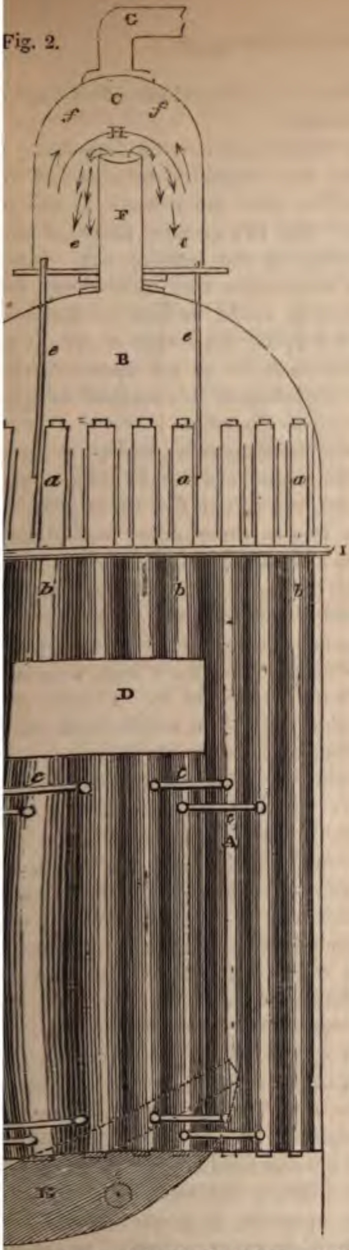
† If the feed-pump is surrounded with water, as is inevitably the case with condensing-engines (and only such are used in this country for steam-navigation), atmospheric air cannot get into the boiler. Upon inquiry, I find that nearly all the feed-pumps used in America, *are worked without having* water outside the pump. This undoubtedly is *one of the reasons why there have* been so many more accidents in America than in *England.*

Fig. 1.



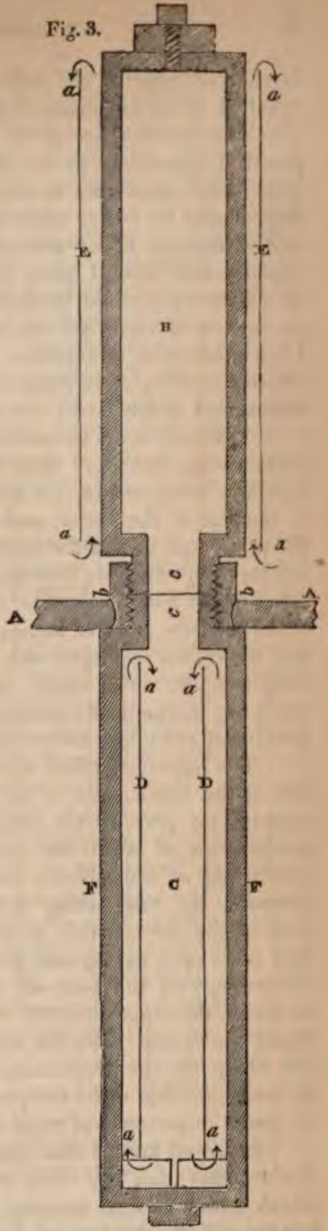
A A'	Sectional view of the compound tubular boiler.
B B	Bottom of the boiler.
C	Smoke box.
D	Fire box formed by elongating the tubes which are attached to the bottom of the boiler B.
H	Steam chamber.
bbb	A view of the tubes which constitute one side of one of the flues and fire-box, there are eight rows of these vertical tubes among which the heat plays. The interior tubes are about one inch apart to allow the flame to play around them; the manner of fixing these tubes is described in advantage 12th, as well as their use.
aaa	Evaporating tubes.

Fig. 2.



w of a locomotive engine's fire-box.—B Sec-
he interior of the boiler.—C Steam-chamber.
—E Swing grate which may be turned on its
he fuel, &c., may be instantly discharged, and
ily removed.—F Steam-pipe for directing the
the concavity of the dome H, as described in
ntage.—G Steam-pipe.—II Bottom of the
ch is screwed the fire and evaporating tubes as
dvantage 12th.
rating tubes *as described in advantage* 7th.—
*es.—cccc Connecting tubes leading from the
ve been cut to form the fire door.—dd* Arrows
ection that the water and foreign matter takes.
ubes.—ff Arrows showing the course of the
parted with its water and foreign matter.

Fig. 3.



Sectional Diagram of one of the many Generating Tubes.

A Bottom of the boiler.—B Evaporating tube standing upright in the interior of the boiler.—C Fire tube hanging perpendicularly over the fire.—DD Circulating tube standing in the interior of the fire tube.—EE The circulating tube surrounding the evaporating tubes.—F Fire-tube.—G The sealing-plug, with a small hole in its centre to receive the fusible metal.—aaaa Arrows showing the course of circulation.—bb Female coupling-screws.—cc Male connecting screws.

overheated part of the boiler, and the tremendous effect can only be equalled by an explosion of gunpowder.

The construction of the new boiler may now be described; but the practical objections to the tubular, the compound-tubular and the common boiler must also be described, so that the remedy to the practical defects may be better understood. The two greatest practical objections to the tubular boiler are its furring up and burning out. After great expense and time, I came to the conclusion that until these two practical difficulties could be removed, they would be fatal to the economical generation of steam for any other purpose than that of steam-gunnery. I have, however, at last been so fortunate as to hit upon a modification which has completely removed all objection to this method of generating steam, and which I will now attempt to describe.

This new boiler is made up of generating tubes and the common flat-bottom waggon-boiler; from this flat bottom a series of tubes hang perpendicularly over, and in, the fire, from one to two feet in length, according to the size of the boiler, and from two to three inches in diameter. On the upper side of this flat bottom is a continuation of these tubes, projecting the same distance into the water in the boiler. In the interior of the tubes which hang in the fire, is fixed a thin tube, two inches in diameter; when the tube is three inches, internal diameter, open at top and bottom, and ten inches in length, this tube stands upon three legs, each, one inch long, and the water stands level with the top of it. These generating-tubes are hermetically sealed, so that the steam which is formed in the interior of the upper half of the tube, cannot possibly escape.

The important effect of circulation is more apparent in this modification of the boiler, than in any other which I have tried. The upper, or evaporating part of the hermetically sealed tube, contains steam of a temperature of about 80° above the boiling point, when the steam is generating at atmospheric pressure; but when generating at a higher pressure, the evaporating point increases in a geometrical ratio. This part of the tube, which is surrounded with water, is incased in a very thin tube, open at top and bottom, which causes a very rapid circulation, and sweeps off the heat so effectually, as to be certain of keeping the steam in the upper part of the tube, at the evaporating point. Experience shows that, after the steam begins to form, not only the fire part of the tube, but the evaporating part of it, which is in the boiler, receives no more addition to its temperature, not even one degree,—which proves the great importance of rapid circulation.

It is well known that water is a worse conductor of heat (particularly downwards) than any other matter; but, at the same time, the property which water has of carrying heat upwards, is greater than any other matter. Now, this law of the upward-carrying power of water is taken advantage of, and by filling the tube about one-third full of water, the steam which is generated is given off at the top of the internal tube, and will constantly keep the evaporating chamber filled with steam, of a temperature in proportion to the density of the steam in the boiler. The effect of the most intense heat serves only to generate steam the faster, *without raising the temperature* of any part of the boiler, generating-tubes, *or steam; while, without* circulation, the boiler would, as is often the case with other tubular Boilers, get red-hot, and generate less steam, by driving off the water from contact with it, and materially injure the boiler. So long as there is enough water in the bottom of the boiler, to be above the bottom of the circulating-tube, say two inches, no derangement of the tube can take place, as the steam and water will, although it is obliged to rise twelve inches, sweep off the heat from the evaporating-tube, which will prevent an explosion of the tube, and which would inevitably take place, when the boiler gets empty or dry, were it not, that in the centre of the sealing-plug is affixed a fusible metallic plug, which is riveted into it, and will melt before the steam is sufficiently powerful to burst the tube.

For marine and locomotive purposes, it has been found that brick-work must be dispensed with, on account of its weight and bulk: of course the fire must be made within the body of the boiler. Now it so happens, that this new modification of the tubular boiler is extremely well calculated for an internal fire-place; for we have only to extend the outward row of tubes down to the fire-bars, and we have the most convenient and economical fire-box.

Second Advantage.—Although it is not yet accurately ascertained what the saving of the fuel is, yet, from repeated experiments, I have no doubt that it will amount to one-third of the fuel now used by the best marine boilers.

Third Advantage.—The reduction of boiler room is owing to the greatly increased evaporating surface in the boiler, which allows much reduction in size, and for the same reason, in weight.

Fourth Advantage.—In consequence of the interior of the boiler being filled with evaporating tubes, which displace a large portion of the water, as well as the reduced size of the boiler itself, it is not too much to say, that one third of the water commonly used will be sufficient.

Fifth Advantage.—In consequence of there being no possible escape from the hermetically-sealed tubes, there cannot be any deposit, as the same water in the generator may be worked over and over again, *ad infinitum.*

Sixth Advantage.—The furring up of the common boiler is occasioned by the sluggish circulation of the water in the boiler, and the extra heat at the bottom of it. But forced circulation not only takes up the extra heat, but keeps all the foreign matter in motion, and as there is a much more rapid circulation at the fire-end of the boiler than at the other, all the matter that would otherwise deposit and become fixed, finds its way to the other end, and can be drawn off by a stop-cock at pleasure, as it will never incrust.

Seventh Advantage.—The generator cannot get above the evaporating* point, since the extra heat is for a certainty swept off by the rapid circulation.

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* To prove the best temperature to generate steam, I prepared an iron cup, of massive thickness, cast for the purpose; it was heated to a white heat, and, whilst it was allowed to cool gradually, several measures of water were placed in it, one at a time, each in succession, as soon as the previous one had evaporated to dryness.

The 1st measure in evaporating, occupied 90 seconds.
2nd 80
3rd 59

The vapour, or steam, thrown off, began now to appear, and became more distinctly visible with the evaporation of succeeding measures of water.

4th measure in evaporating, occupied 30 seconds.
5th 20
6th 12

Eighth Advantage.—Experience shows that wherever circulation is active, no heat can get above the evaporating point, let the heat be ever so strong*. This boiler is so constructed that no part of it is exposed to strong heat, where strong circulation is not at the same time going on; consequently no over-heating can by any means take place. It is a fact, that no extra heat can get into the steam, since no heat is suffered to pass into the boiler above the water, let it get ever so low.

Ninth Advantage.—The tubes of the locomotive tubular boilers now in general use, are riveted at each end; and as no provision is made for guarding against expansion and contraction, the wear and tear is enormous. The tubes, however, in this boiler are connected in the middle, and each half is allowed to contract and expand without impediment.

Tenth Advantage.—To separate the steam from the water and foreign matter, a small steam-chamber is attached to the top of the furnace-end of the boiler. A pipe somewhat larger than the steam-pipe passes from the top of the boiler to the bottom of this steam-chamber. Directly over this pipe, a dome is fixed, about three quarters the diameter of this chamber; the depth of this dome is rather more than half a sphere, and within two inches of the top of the pipe. From the bottom of the chamber there is also fixed a return-pipe, half the size of the steam-pipe, which leads down to within two inches of the bottom of the boiler. The operation is thus. When the steam rushes into the chamber, it takes with it more or less water and foreign matter (this is what is technically called priming), which strike the concavity of the dome; the dome throws down the water and foreign matter to the bottom of the chamber, while the steam in a pure state passes off through the steam-pipe, and the foul water returns to the bottom of the boiler through the return-pipe.

Eleventh Advantage.—The steam is got up much quicker than in any other boiler, in consequence of the great evaporating surface within it, and the diminished quantity of water in the boiler.

Twelfth Advantage.—The construction of this boiler is extremely

7th measure showed what I had termed the evaporating point, and in a dense cloud of steam, evaporated suddenly in 6 seconds.

8th measure occupied a longer period, viz., 10
9th measure in evaporating, occupied 20
10th 32
And the 11th measure did not boil.

The first measure of water, although contained within the iron cup at a white heat, was perceptibly not in contact with the metal, but was repelled to some distance from it in a state of buoyancy, and there moved freely in every direction. So circumstanced, the water evaporated slowly; but when, by the evaporation of successive measures, and the lapse of time, the iron was cooled down to the "evaporating point," the water then evidently came in contact with the iron, and the augmented rate of evaporation was as 90 to 6, or as 15 to 1, the rate being increased or multiplied fifteen times; or, in other words, a given quantity of water was converted into steam, fifteen times quicker at a moderately low, than at an intensely high, heat.

* It is a curious fact, that there are now many boilers which have been in constant use for more than fifty years,—the cause is, that these boilers are sufficiently large to make all the steam required, without being forced; this is done with a great sacrifice of fuel: but since it became necessary to economize fuel, the boiler has been very much reduced in size and altered in form, exposing many parts to be overheated. It is true, such boilers raise much more steam with the same fuel; and undoubtedly much more is saved in fuel than is lost in wear and tear of the boiler. This is noticed, to show the great advantage of so constructing the boiler that the heat will always be kept down.

simple: the bottom plate, after having been perforated with proper-sized holes, female coupling-screws are firmly riveted into it; the lower half of the tubes, which have been reduced one-third in size, about two inches from their ends, is formed into a male screw, to fit the female coupling-screw. This male screw is faced perfectly flat, and the shoulder is made to be screwed firmly in contact with the bottom of the boiler. The upper half is screwed in the same manner. The face of this screw is rounded, so that when it is brought in contact with the flat surface of the lower half, it may be the more certain to make a perfect joint. The upper half is not allowed to touch any part but the flat surface of the lower half of the tube. The plug-nut, which is used for hermetically sealing up the tube, is perforated in the centre with a small hole,—say one-eighth of an inch in diameter, and filled with a fusible metal, which will be driven out before the tube will rend, and which could only take place should the water be allowed to escape from the boiler.

The Waggon Boiler is considered the weakest form. It will be seen, however, by the diagram, that this new boiler is altered somewhat in shape; the bottom is perfectly flat instead of concave; the sides are also flat; the top is semicircular. The female coupling-screws undoubtedly materially strengthen the flat bottom. The boiler is to have tie-bolts from the top, the number of which is to be determined by the strength of the steam to be generated in the boiler; they pass down vertically between the tubes, and are screwed into the flat bottom of the boiler. Tie-bolts are to be used also to hold the flat sides of the boiler from bulging out when used for high steam. None of the nuts of the tie-bolts are exposed to the fire, consequently no objections can arise from that source. This boiler may be made much stronger than any other, on account of its diminished size; setting aside the absence of any danger from the second and third cause of explosion, which has been described, the ends of this boiler, which are flat, may be made sufficiently strong by ribs. In fact, this boiler must be pronounced a perfectly safe one, since only the first kind of explosion can take place, which is absolutely harmless; the first kind has also been described. The ease with which this boiler can be repaired is not one of its least recommendations. Duplicates of the tubes may always be at hand, and if any give way, from unsoundness or any other cause, they can be readily replaced, as they are fac-similes of each other.

Thirteenth Advantage.—All persons who have been in the habit of using anthracite-coal, know that the intensity of its heat is so great, that if urged to its greatest power, the best fire-brick is readily fused. It is on this account that it is so difficult to be used for raising steam; still, some careful stokers have used it to great advantage. It is, however, done at a great sacrifice of heat,—for slow combustion and thin firing only will answer. To produce the greatest effect, rapid combustion, with a deep fire is necessary. In the new boiler, the heat cannot possibly be too great. This coal, which is called in Wales, stone-coal, may be obtained there in any quantity, and is undoubtedly the most economical where it can be used, as is the case with this boiler.

JACOB PERKINS.

REVIEW.

I. *On the Construction of Coaches*, by Sir Henry Parnell, Bart. M.P. Nov. 1835. Folio, 12 pp., 2 pl. Printed for gratuitous Circulation.

A judicious and well-timed enumeration of the points of desirable reform in the construction, &c. of our stage-coaches. Hitherto the errors and prejudices of coachbuilders and wheelwrights (except the latter in Edinburgh and its neighbourhood,) appear to have been unconquerable. The dished-wheel and the bent axle are still exclusively manufactured in the London workshop and the village shed; and though "the roads are now no longer cut into deep ruts and holes, the coaches are all nearly as heavy as they were fifteen years ago!"—p. 10.

The suggestions of the author, for the removal of these and other objectionable practices almost as universal, are detailed with perspicuity, and explained by plates, containing elevations of the side and back, and a transverse section, of an improved coach.

The principles which are the basis of the proposed improvement are fully given; but the application of them is advised to be carried no further than experience will justify; and it is demonstrated, that this valuable guide has long shown, that in a perfectly-constructed coach there ought to be

The greatest possible height of all the wheels.
„ „ depression of the body.
„ „ shortness of the perch.
„ „ length of axle-trees.
„ „ diminution of bearings and tires.
„ „ length and pliancy of springs.

That these conditions are not yet fulfilled by our stage-coach builders, and that they may be made "without departing very much from the present plan of building coaches," the author shows by a "*comparison of the size of a coach built on the proposed plan, and of a common coach.*" (p. 10.)

Every stage-coach passenger (and who is there now, male or female, that is not, occasionally at least?) will sympathise with the following remark, and the proposed remedy.

> The convenience of travellers has been very little attended to in arranging the size of the bodies, and the height and depth of the seats inside coaches.
>
> The object should be to enable the traveller to sit in such a position as will admit of his performing a long journey with the least fatigue.
>
> A slight degree of consideration will make it clear, that the seats should be so high as to give ample room for the legs, and thus allow the whole length of the thighs to press upon the seats, and be supported by them. If the legs have not room to hang perpendicularly, and are therefore thrown forward, the fore part of the thighs are elevated, and the weight of the body is made to rest on the upper part of them, so as to produce a partial strain on the muscles in supporting the body.
>
> In addition to this, the body is thrown backwards, and the weight of it is *thereby supported with* a considerable strain on the muscles of the back

which leads to fatiguing them. There can be no doubt that a person who sits on a high, deep, and broad seat, and in an erect posture, as he does naturally on a well-formed high chair, will suffer less fatigue, than one who sits on a low seat, and in a reclining posture.

The popular prejudice in favour of the load being as forward as possible, Sir H. P. attacks. He says,

> According to the established law of mechanics, that the higher the wheel the easier a weight upon it is drawn, when a carriage has two sets of wheels, one of which is necessarily lower than the other, the body of such a carriage should be so constructed, and so placed over the wheels, that the greatest possible portion of the load should be over the hind wheels.

And he advises:

> In order to keep the weight of luggage as far behind as possible, nothing but carpet bags and light packages should be put into the front boot. As the top of the coach will be only seven feet six inches from the ground, portmanteaus and other heavy luggage may be put upon the roof. If three feet three inches of the roof next the guard's seat be used for this purpose, the weight of the luggage on the roof will be over the hind wheels. For still further securing the object of having the weight as far behind as possible, a strong iron should project, one foot from the bottom of the hind boot, having an iron frame, eighteen inches in length, fastened to it with a hinge, so that extra mail bags, and heavy luggage, may be packed to the full height, if necessary, of the top of the guard's seat.
>
> In packing coaches, the hind boot should be first packed, and then the hind irons. The fore boot should be used only for parcels and luggage to be dropped or taken up on the road.

On two other disputed, but important, points, the Author says,

> As the practice of dishing wheels, and bending the ends of the axles downwards, originated in some degree in the bad state of the roads, now that they have become so much improved, the bending of the axles should be wholly laid aside*; and the degree of dishing given to the wheels should be reduced, and made wholly to depend upon experience, with reference to the wearing out of wheels, and the expense of renewing them.

The above are the principal points insisted upon; and if the alterations they demand in the construction, arrangement, capacity, weight, and loading of stage-coaches, were carried out into practice, the following most valuable results, among others, would be obtained:—

1. A diminution of the labour of the horses; and,
2. An increase of the convenience, ease, and safety of the passengers.

Though Sir Henry Parnell bears his own testimony to the practicability and necessity of the proposed improvements, (and probably more unexceptionable evidence could not possibly be obtained,) he has thought it right to support his assertions by extracts from scientific writers; but wishing to strike a still heavier blow at the very root of the evil, by clearing away the ignorance of our wrights and builders, through the means of actual trial and experiment, he adds,

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[* A slight inconsistency has escaped the attention of the author on this point; following out sound principle, he here advises the total banishment of the bent axle; but, at p. 5, he recommends, *in order to obtain* a particular effect, that it shall still be used, *though with a reduced droop.*—*Ed.*]

Although the extracts, which have been taken from works of science, are quite sufficient to convince all persons who have received a scientific education, that the fore wheels of a coach ought to be high, and that the greater part of the load should be placed over the hind ones, as it happens that few of those persons who are concerned in the directing of the building, and in the building of coaches, have ever applied themselves to scientific inquiries, so as to know either why spokes are called levers, and what the property of the lever is, or what the effects are of the friction of wheels in turning on their axles, and in moving on roads, it is quite necessary that experiments should be made, so that by showing how much work horses actually do in drawing different kinds of carriages, nothing shall be left wanting to expose the prevailing errors, with respect to wheels, and the proper manner of loading coaches.

The invention, by Mr. M'Neill, of an instrument, for trying the draft of carriages, which has been found to be perfectly fit for the purpose, now admits of such experiments being made, with a certainty of leading to accurate results; and it is very important that they should be made.

As this instrument can be fixed to a coach, with the horses to it, what it shows is, the actual force or labour which they exert in drawing; and, therefore, the experiments made with this instrument are not liable to errors, like other experiments, where it is necessary to use a substitute for the real power.

The expense of making a proper set of experiments would amount to some hundred pounds; as, however, there exists nothing to make it worth the while of any private person to incur it, these experiments should be ordered and paid for by the Government. This small expenditure would soon be repaid by the saving which would be effected by diminishing the labour of horses in drawing stage coaches, and, consequently, the expense, which now falls indirectly on the public in providing a sufficient number of them, and maintaining them.

We are informed that Mr. M'Neill has, at the request of the Prussian Government, lately constructed an improved instrument of this kind, probably the most perfect of any yet made. This request of a distant nation, may enable us to form an opinion of the vigilance and activity of the part of the administration of that country which superintends the maintenance of the public roads and carriages. And shall England be behind in the further examination of a question of such importance to its commercial prosperity? Shall a country, in which so large a part of its population is, day and night, continually on the move, be still unable to state, on undisputed grounds, what is the best form, and weight, and construction of a safe vehicle of transport? It might have been very difficult to have done this even a very few years ago; but now, when so experienced a person as the author of this work states, that we have, at the present moment, all the data and the instruments necessary, and that the solution of the problem merely waits for a moderate pecuniary aid from the State, let us hope that our ignorance will not be permitted to continue much longer.

II. *Mathematical Researches.* Parts I., II., and III. By G. J. Jerrard, A.B., Bristol. London, Longman and Co.

Under this very wide and unexplanatory title, Mr. Jerrard has given to *the world the fruits of* a most extensive and profound investigation of the theory of algebraic equations, not only in their numerical but in their most perfectly general character. Of the value of these researches, no doubt can be entertained among mathematicians; but it is not every student who will be able to profit by them, as in addition to the difficulties of the subject, it has been necessary, in order to express the very comprehensive views of the author, to adopt a new and peculiar notation. It is a proof of their value, that on an account of them being given this year to the Physical Section of the British Association, a sum of money was immediately voted, in order to have some of the prolix arithmetical operations executed, which are necessary for the full developement of the theory.

III. *On the Theory and Solution of Algebraical Equations.* By J. R. Young, Professor of Mathematics in Belfast College. 12mo. London, Souter.

Professor Young has been long and advantageously known to mathematical students, for a series of accurate, elegant, and perspicuous elementary treatises, forming a course of pure mathematics. The present volume supplies the only portion of the course which could be considered deficient: and is marked by the peculiarity of embodying, for the first time in an English work, the recent researches of M. Sturm, which, involving the discovery of a remarkable theorem of the utmost generality, supersede the methods of preceding algebraists, and complete the investigation of the theory of numerical equations.

IV. *Minerals and Metals, with their Natural History and Uses in the Arts.* London, J. W. Parker.

This very small volume will be read with great interest, not only by the young, for whom it is specially intended, but by those of all ages, who have not specially made these subjects the object of their studies. We have here, in a very small compass, a mass of useful information respecting those materials of all comfort and civilization, (as the mineral products of the earth may truly be called,) conveyed in an agreeable and attractive manner, and illustrated with many well-executed wood cuts.

V. *The Student's Cabinet Library.* No. II. *On the Connexion between Geology and Natural Religion.* By Professor Hitchcock, Amherst College, U. S. 12mo. Edinburgh, Clark.

Though there are some few subordinate points in the author's argument which seem to us defective, yet, upon the whole, we consider this an admirable and useful tract, on a subject of the most serious importance. It stands pre-eminently distinguished by sound sense and enlightened philosophic views, amid the mass of publications on the same subject, which the present age has produced: many of which, with the best intentions, we do not doubt, on the part of their authors, from the ignorance, bad logic, and worse taste, which they display, are eminently calculated to expose to the scoffs of the sceptic, the sacred cause which they advocate, instead of affording it *any rational* support.

VI. *Means of comparing the respective Advantages of different Lines of Railways; and on the Use of Locomotive Engines. Translated from the French of* M. NAVIER, by JOHN MACNEILL, C. E. 12mo 96 pp. London, Roake and Varty.

THE discussions which took place in 1835, before Committees of the two Houses of Parliament, upon the Bill for the Great Western Railway, will ever be remembered as an epoch in the history of English civil-engineering, and its professors. A fact was then elicited which may lead to many a curious theme of inquiry and reflection as to its protracted existence, but which certainly must force those who have the direction of the expenditure of the enormous sums which are annually devoted to the construction of railways, to be better prepared for future inquiries of the same nature.

The bill above mentioned was opposed; and the most formidable source of opposition was the proposition of a rival line. A comparison of the two lines became, therefore, a very important duty of the committees. This was prosecuted successfully, without more than the usual difficulties, through most of the points of collision which arose, and one only remained which, though equal to any in importance, yet as it was a subject of mere calculation, we should not have expected, at this advanced period of railway experience, it could have raised a question among the learned engineers. The case was cleared of all questions of local interests, &c., and reduced to this simple form :—A railway for the transport of loaded carriages is desirable between two places, A and B; two lines of country are presented for this purpose, and each recommended by its respective patrons as the most eligible; the one passes from A, through a point C, to B, and the other through a point D, so that the lines are represented by ACB, and ADB. Now, by which of these lines can a passenger or ton of goods be carried cheaper or more expeditiously? Will the law-making,—will the scientific,—will the speculative,—will the mechanical,—will the professional world believe that no answer was given in these Committees to this question! that none could be obtained! that the bill passed into an act without the information it would have given! that in all inquiries which ever have been made into the merits of rival lines of railways by proprietors, opponents, committees,—parliamentary and others, &c. &c., the question, if asked, never was answered! and that at that time no book existed in the English language which contained the data by which the answer could be satisfactorily stated; that is to say, not a table, formula, or statement, by which an engineer could calculate the cost of transport of a given load upon a railway of given length and given rise of inclination! It is true, that in the committee, opinions were given, that the line ACB was preferable to ADB; but these were met by opinions, that the line ADB was preferable to ACB; and the committee was expected to be influenced by the weight ("if that was *weight*, which *weight* had none,") of these conflicting opinions, in the absence of all facts and calculation founded upon accurate data. These *opinions*, and the skilful shuffling and mystification, which ignorance and cunning can, at times, bring successfully into play, cost the parties interested in the Great Western Railway question, a very large sum of money, without the slightest useful consequence to mitigate the loss, while the problem might have been solved, and the truth demonstrated, with mathematical precision, by a single competent witness, at the cost of about half an hour of his time!

The dispute in question was continued week after week; thousand after thousand was expended; and, when a little light did once strike across the gloom, ignorance, as usual, was alarmed, and, before it could be more fully developed, she succeeded in driving it from the arena; but heads have cleared since, and inquiry has exposed more completely the deficiency in some of our professional crania and libraries.

The humiliation produced in all the honourable minds of the profession, we suppose is deep; and we are pleased to see that, by the means of M. Navier's elegant investigations, the country may be saved from similar disgrace in future. The means this gentleman presents, are the only ones which can suit the case; viz., a thorough mathematical mastery of the subject, by which the true result in every particular instance can be obtained. This will banish mere opinion from the witness-box, will shorten inquiry, furnish solid grounds for decision, and save much valuable time and labour. We confess our patriotism would have been gratified, if Mr. Macneill had presented us with an original work on a subject so eminently national, as that of railways. As it is, he has preferred to introduce to English readers the labours of a French mathematician, distinguished for his application of science to this branch of mechanics.

In some notes with which Mr. M. has prefaced his translation, he makes a suggestion, which is so in harmony with the principles upon which this Journal, and the Gallery with which it is connected, have been founded, that we give it at length.

> As practical men have seldom the time, or the mathematical acquirements that are requisite for such an intricate investigation, a series of *facts* should be registered at all the different railway establishments in the empire. These *facts* should embrace, 1st, the cost of the engines, their weight, a daily journal of their repairs, and the number of miles travelled daily; the weight carried, the water and fuel consumed, the pressure of the steam on each particular part of the line, &c. &c.; if such facts were collected, and given to some of our able mathematicians, there can be no doubt of their being able to furnish formulæ that would be of the utmost importance to the country: or it might be better to employ such persons to design and carry on, under their own superintendence, sets of experiments, in any way they might think most adapted to obtain the information desired. Such persons might be easily found amongst our mathematical professors, and the expense of the experiments should be defrayed by the Railway Companies, in some certain proportion.

The perfect execution (and the imperfect, would be mischievous,) of such a proposition would be of the most extensive benefit; and the cost, if distributed as the author points out, absolutely insignificant. Our humble assistance shall be at all times ready in the furtherance of this and similar accumulations of practical information.

In the little work under consideration, the aim of M. Navier has been, to point out the greatest amount of reduction, which the establishment of a railway can effect upon the actual cost of transport, so far as can be obtained by mathematical investigation, founded upon data

We earnestly recommend our young engineers and mechanicians, to lay aside the alarm they generally express, when the study of the calculus is recommended, and to make an effort, if merely as an experiment, to read this work. We venture to predict, that if they do, they will feel that a film has fallen from their eyes, and that their right hand has increased in cunning. New and unexpected scenes of enterprise will open upon them, and an exquisite sensation of newly acquired power will irresistibly urge them to enter, and gather part of the immense harvest which yet awaits the sickle of the accomplished reaper.

There can be no doubt, that facts in the processes of art, and in the operations of nature, extremely simple in their appearance, but, perhaps, of immense importance, either for application or explanation, are daily passing unobserved, or if observed, soon forgotten and lost. The latter principally proceeds from the incapability of the observer to reason upon such phenomena, even when his natural sagacity teaches him to suspect, that they may be far more valuable than mere novelties. No class of men are likely to be so fortunate in this respect, and, if ignorant, to be so perplexed, as the rising engineers and mechanicians of the age. The powerful natural agents now under the control of man, and ministering in a thousand ways to his convenience and gratification, are daily presenting new data; from which, also, are daily proceeding new trains of reasoning and research, pointing towards results which, but a few years previous, even the human imagination had not dared to approach. Such results, however, demand the calculus for their easy and complete attainment.

To return to the work under consideration, we cannot completely discharge our duty, without stating our regret, that the author has omitted the answers to the exercises, and has placed the explanation of the signs used in the work, at the end of it, without any notice of them in the contents. The first, we are disposed to consider, as a material deficiency, and will be particularly felt to be so by the remote and solitary student. There may be some objection to placing an answer immediately after the proposition: the premature conception of it, so given, may injuriously influence the inquiry, and substitute for the mental exercise intended, another of a totally different character; but this will not apply, at least in the case of an honest inquirer, to placing it in an appendix. The answers are sure to be demanded even by the most able student, and a key will, therefore, be wished for, probably long before it can be obtained; and not then, probably, without additional cost. When it is published, we fear we must request of the learned Professor to elucidate his "illustration" at p. 18:—proceeding "step by step:" there is nothing preceding this illustration that enables the *learner* to see that $u=\frac{ax^3}{3}$, and at the same time $=\frac{12}{3}\times 10^3\times 2$.

The book is very well printed, and the price not unreasonable; but we wish, on account of a large class in whose hands we could desire to see it, that it had been possible to have published it at much less.

MISCELLANEOUS INTELLIGENCE.

Annual Depth of Rain in England.

A VERY accurate observation has been regularly made for several years of the rain which has fallen at Kendal, in Westmoreland; and we are enabled by the kindness of Mr. Wakefield, a resident, to present a table of the quantity which has fallen in that neighbourhood during each month for the last seven years. The depth of last year it will be seen, amounted to $55\frac{1}{10}$ inches, which is very nearly the mean of the six years previous.

It would be interesting and useful to compare this return from a country of mountain and lake, and situated in the northern part of England, with others made as carefully, from districts of different surfaces and positions throughout the three kingdoms, and we shall be obliged by receiving such; and if accompanied by a description of the instrument, their position with regard to the ground, &c., and other details, they will be still more acceptable.

ANNUAL MONTHLY DEPTHS OF RAIN WHICH FELL AT KENDAL DURING THE LAST SEVEN YEARS.

	1829.	1830.	1831.	1832.	1833.	1834.	1835.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January . .	0·747	0·429	1·619	2·278	1·628	14·758	5·349
February .	1·234	4·774	8·208	4·258	4·582	5·723	8·820
March . . .	0·867	5·045	6·028	3·549	2·070	5·171	5·049
April . . .	3·511	5·656	2·433	2·235	3·754	1·043	1·589
May	1·977	2·831	0·721	1·602	2·534	1·637	3·063
June . . .	4·204	5·289	2·682	4·643	7·715	6·699	1·254
July	5·569	4·961	4·081	2·639	2·233	5·048	6·259
August . .	9·383	4·218	3·899	4·433	1·966	6·167	3·107
September .	5·243	8·027	6·393	2·295	3·527	4·908	7·815
October . .	6·684	4·695	11·812	8·346	3·752	4·715	4·386
November .	3·855	10·023	8·560	5·373	7·438	4·206	6·311
December .	2·899	2·082	4·980	8·037	14·219	5·047	2·889
Whole Year	46.173	58.030	61.416	49.688	55.418	65.122	55.891

Mean depth of the Seven Years . . . 55·962 Inches.
Total ditto Do. . . . 391·738 Do., or 32 Feet, $7\frac{3}{4}$ Inches.

Aurora Borealis.

THE Aurora on the evening of November 18th, 1835, was witnessed with great brilliancy at Oxford. A low, broad, luminous arch stretched along the N.W. horizon at its centre, not rising to any great elevation: this remained quite stationary, but from it other bands or streams of light shot up. One observer saw a number of them together, which ascended to the zenith, and converged like the ribs of a dome. Others saw only a few, perpetually shifting, but of no great height; whilst one stretched upwards to the zenith, and beyond it, and was continually flickering and waving.

Curvilinear Direction of Winds.

CAREFUL and continued observations, contained in the annual reports furnished by the several academies in the state of New York, to the Regents of the University, appear to demonstrate the fallacy of the notion commonly entertained, that winds are generally rectilinear in their progress, and blow for the most part in right lines over extensive portions of the earth's surface; an error which appears to remain undisturbed in the minds of most meteorologists.

Temperature of Canton and Macao.

Mr. **Meyen**, for some time a resident at Canton and Macao, states, as the result of his own observations, and those of other residents during very considerable periods, that the mean temperature of Canton is 71½° Fahr. and that of Macao 72¼° Fahr. It will facilitate accurate comparison to remark that the mean temperature of London, as stated by Professor Daniell, is 49½° Fahr.

Filtration and Cooling of Light.

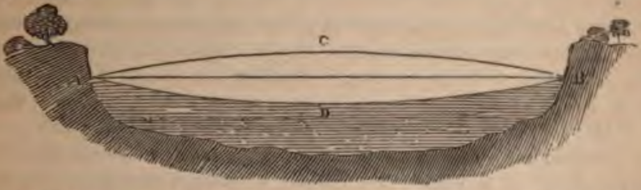
M. **Melloni**, in examining the correctness of his opinion, that light and radiant heat were produced by different causes, and that there was therefore a possibility of separating them from each other when combined, has succeeded in accomplishing this remarkable experiment. By a process extremely simple, he separates light from radiant heat, whether proceeding from ordinary fires or from the sun. His mode is this:—The radiation from a luminous body is passed through a system of diaphanous bodies,—these absorb all the radiant heat, and extinguish but a very few of the luminous rays. The pure light emerging from such a system is found not to affect the most delicate thermoscope, even when concentrated by lenses to a brilliancy equal to that of solar light. The substances hitherto employed in this heat-absorbing system, are water, and a peculiar kind of green glass, coloured by oxide of copper. The cooled and filtered light, as it may be termed, is decidedly yellow, with a tint of bluish-green.

Periodic Appearance of Shooting Stars.

M. **Arago** has recently given publicity to the notion that millions of groups of opaque bodies floating in space, may in their periodical revolution, annually cut the path of the Earth near that point of the Ecliptic where our planet may be found about the middle of November; and that on entering into our atmosphere, these bodies may inflame, and so become visible to us. He draws this conclusion from the reports of several observers of different nations, who have described appearances of this class of meteors which were remarkable for numbers and brilliancy; but more particularly for the circumstance, that though observed in different years, they all occurred on or about the 13th day of November.

Velocity, &c., of Currents in Rivers.

M. **de Fontaine**, an engineer long employed upon the Rhine, has published an elaborate work descriptive of his actual operations upon that river during twenty-one years. In it the author states the result of his numerous experiments, to determine the law of the variations in the velocity of the current of a river, which take place between the surface and the bottom. He finds the line of mean velocity always to be upon the surface when the bed of the river is clear, smooth, and continuous; and that it descends and approaches the bottom, in proportion as the bed is irregular, and as the number of obstructions lying upon it is increased. M. de F. has also ascertained, that a line, supposed to lie upon the surface of a river, at right angles to its shores, and free to move in a vertical plane only, varies very much in its form, though generally supposed to be straight and horizontal, as A, B, in the annexed figure. This, M. de F. shows is never the case, except when the flow is uniform, and the height of the water is permanent; but that when the river is rising, this transverse line becomes more or less convex, as A, C, B; and, that on the contrary, it dips in the middle, and becomes concave, as A, D, B, when the river is falling.



M. de F. suggests, that the well-known differences of opinion which exist between professional men on this subject, may be principally owing to one or other of these particular cases only having been observed, and erroneously considered as a general law. The instrument used in these experiments on the velocities, was invented by M. Wattmann, an accurate account of which we should be happy to receive for the purpose of publication, and of comparison with one lately constructed in this country for the same object, and which will be described in our next number.

Variation of Temperature in Rocky Strata.

Mr. W. HENWOOD conceives that he has satisfactorily ascertained that a difference of 2°—3° Fahr. exists in the temperature of the schistose and granitic strata of Cornwall, when they are severally examined at the same depth. It is not stated to which the higher temperature belongs.

Reflected Heat measured.

THE fact that heat is reflected more or less abundantly in proportion to the nature and polish of the surface upon which it impinges, was confirmed by the researches of Rumford and Leslie; but these philosophers did not proceed to ascertain the proportion, in each particular case, of the incident, to the reflected, heat. It is easy to imagine a variety of cases in which, the property of the reflection of heat being known, and also that its quantity was variable, it would be desirable, and often exceedingly useful, to be able to ascertain the amount which could be obtained from any particular body and surface. M. Melloni has recently shown, that by means of an apparatus, designed by him, the problem can be solved with great accuracy. Another instance in which the genius of M. Melloni, aided by the exquisite delicacy and sensibility of his apparatus, has detected and exhibited properties and proportions of this invisible and universal agent, which appeared a short time ago to lie far beyond the utmost reach of the powers of man.

Flowering of a West India Plant in the Open Air.

AT a Meeting of the Ashmolean Society of Oxford, on the 6th of November last, Dr. Daubeny exhibited a specimen of the *Bromelia pinguis*, a native of the West Indies, which flowered last autumn in the open air in the garden of Mr. Shirley, of Eatington Park, near Shipston-upon-Stour. This plant has rarely blossomed *in Europe* even under glass, although a drawing of it in flower is given in the *Hortus Elthamensis;* and the individual plant alluded to had been tried first in the pinery, and afterwards in the greenhouse, but had never put forth flowers, till it was taken out of doors, when it flowered, though the petals never properly expanded.

Phosphoric Light emitted by Flowers.

AT the same meeting, a communication was also read by him respecting an electrical phenomenon, which occurred in the garden of the Duke of Buckingham, at Stowe. On the evening of Friday, the 4th of September, 1835, during a storm of thunder and lightning, accompanied by heavy rain, the leaves of the flower called *Œnothera macrocarpa,* a bed of which is in the garden, immediately opposite the windows of the manuscript library at Stowe, were observed to be brilliantly illuminated by phosphoric light. During the intervals of the flashes of lightning, the night was exceedingly dark, and nothing else could be distinguished in the gloom except the bright light upon the leaves of these flowers. The luminous appearance continued uninterruptedly for a considerable length of time: it did not appear to resemble any electric effect: and the opinion which seemed most probable was, that the plant, like many known instances, has a power of absorbing light, and giving it out under peculiar circumstances.

Beet-root Sugar.

THE exertions making in France and throughout Germany to simplify the process of preparing sugar from the Beet are immense and unceasing. At the recent meeting of the German naturalists, at Bonn, the section of Agriculture and Rural Economy was almost entirely occupied with papers and discussions on the subject. At Valenciennes, a manufacturer has succeeded in discovering a method of crystallizing the whole of the saccharine matter of the Beet without producing molasses in the process. Three sugar-houses there have adopted the new plan.

Science assisted by the State.

THE French government, in October last, being about to despatch a vessel, *La Bonite,* to the Brazils and Sandwich Islands, through the Chinese and Indian Seas, gave permission to the Academy of Sciences of Paris, to take advantage of the circumstance for scientific inquiry. This body eagerly embraced the offer, and appointed M. Arago, and other distinguished members, to draw up suitable instructions for the officers of the vessel; these gentlemen are also stated to be able and willing to second the views of the academy. Instruments, such as the shortness of the time would permit, were immediately collected and prepared. Should not this example stimulate us to seize some of the many hundred opportunities which occur every year in this country, not only in the case of the government, but of the numerous public bodies, companies, and powerful individuals, who are daily sending out vessels to cross every sea, and visit every point of the globe? It is true, we cannot be accused of absolute neglect,—there are a few valuable instances to the contrary; but how rarely do we see a case in which our Royal Society is occupied as the French Academy has lately been!

In the present instance, gratifying and important results have occurred even before the sailing of the vessel, as is proved by the following fact. During the examination and comparison of some of the instruments intended to be sent on board *La Bonite*, some extraordinary irregularities were observed in the direction of the magnetic needle. These happened on the 17th, 18th, and 19th of November. The observers were led to suspect the existence of an Aurora Borealis, but the atmosphere at Paris was cloudy and unfavourable for ascertaining it. In a few days the correctness of the supposition was made evident, and the extensive influence of a remote Aurora upon the magnetic needle was confirmed, by the arrival of the English newspapers, and also of a communication from Nismes, containing descriptions of a brilliant Aurora, which occurred in the night of the 18th of November*. We have little doubt, but that this series of beneficial effects will be continued, and that, barring accidents, the return of *La Bonite* will, in addition to the discharge of her duties to the French government, be accompanied by a gratifying accumulation of knowledge to the French Academy, and through them to the scientific men of all nations, and eventually to the world.

New Electro-Chemical Apparatus.

M. BECQUEREL of Paris, has introduced to the notice of the scientific world, a modification of the Electro-chemical Battery, which reduces it to a striking simplicity. And yet he finds that all bodies exposed to it are either decomposed or attacked, exactly as if they were submitted to the Voltaic pile; that its power of action continues uninterruptedly; and that the intensity of its current is not affected, in any appreciable quantity, by the causes which tend to weaken the electro-chemical effects of the pile.

Simple as M. Becquerel's apparatus is, its arrangement has already been improved by M. Aimé, who describes the newer instrument as follows:—A U-formed tube, pierced with a small hole at its lowest part, is to have its legs half-filled with very fine sand. One of the legs is then to be filled up with diluted sulphuric acid, and the other with a concentrated solution of sea-salt. The two fluids will descend through the sand, and combine at the lower part of the tube. As soon as this combination is effected, the fluid, resulting from the combination, escapes immediately through the hole, which is loosely plugged with a bit of asbestos to prevent the escape of the sand. A slip of platina is dipped into each leg, and wires from these slips connect them with a galvanometer. On making contact, the needle immediately indicates, by the change in its direction, the formation of the current produced by the action of the two fluids on each other. The intensity of this current varies with the degree of concentration of the solutions, and with the rate at which the combination is made; to facilitate the latter, the size of the hole may be increased.

When a constant and regulated supply of the solutions, and a proportionate discharge of them, after they are combined, are obtained; the process can be carried on for any desired period without intermission. There is, however, one precaution necessary,—the solutions must be so prepared and proportioned, that they shall not, on combining, form a salt and obstruct the discharge. From the experiments of M. Becquerel and M. Aimé there seems already ground for believing, that electro-chemical currents of determined intensity may be produced and maintained during whatever time may be necessary to effect chemical decomposition, by this powerful agent.

Duration of Life in France.

IN a recent memoir by M. Demonferrand, he states, that the average length of human life in France is 33 years, 8 months, and 11 days.

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* *See Notice of the* Aurora Borealis, at Oxford, p. 65.

Magnetism as a Motive Power.

The Rev. Mr. M'Gauley exhibited to the British Association at Dublin, a very simple contrivance by which Magnetic force is employed to drive machinery. The magnetism is produced in soft iron by a galvanic battery; and by an ingenious but simple contrivance the connexion of the wires is alternately reversed, so that a bar of iron is kept continually moved backwards and forwards between the opposing poles. This moves a crank, which turns a wheel, to which of course any other wheels can be attached. The extent of power he conceives will be as unlimited as the strength of the battery which is used.

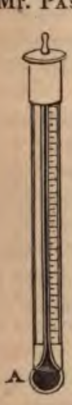
Marine Instrument.

One of the instruments furnished by the French Academy to the officers of *La Bonite*, is intended to procure specimens of sea-water at great depths. M. Biot formerly used an instrument of this nature in the Mediterranean, and obtained sea-water from a depth of about 450 fathoms, together with all the air, &c., that it might contain, for the purpose of examining and comparing it. M. Savart, who has superintended the execution of the new instrument, has introduced some important improvements. We hope to be able to give a description of these improvements from the author himself.

Simplification in the Laws of Chemical Action.

M. Biot, at a sitting of the Academy of Sciences, on the 23rd of November last, opened a sealed communication which he had deposited with the Academy for some time, and read from it an interesting paper, in which he had applied optical principles most successfully to the resolution of the constituent proportions of chemical combination. At the conclusion, he called the attention of his auditory to the extreme simplicity of the new laws he had placed before them; and observed, that he felt justified in thinking that it was probably not so difficult as it is at present imagined, to reduce the phenomena of chemistry to the same simple and exact calculation as those of mechanics.

Convenient Chemical Thermometer.



Mr. Pastorelli, of Cross-Street, London, and Herr Greiner, of Berlin, have simultaneously succeeded in producing an improvement in the thermometer for chemical purposes. The preservation of the scale from the action of acids, and the safety with which the bulb and tube may be used as a stirrer and afterwards cleaned from any viscous or other matter into which it may have been dipped, are accomplished without any diminution of the sensibility of its action. The accompanying figure shows the improvement. The tube of the thermometer is passed up a glass cylinder, of a diameter rather less than that of the bulb; the cylinder can therefore rest upon the bulb, as at A, and is here attached to it by the blow-pipe. The scale is inserted within the cylinder; and the brass cap which terminates it, preserves the tube in its axis. The idea of this arrangement and guard is by no means new, and many thermometer-makers have attempted it, but they never were able to make the joint at A to stand the necessary variations of temperature. Mr. Pastorelli, after a number of experiments made during eighteen years, has at length succeeded, *and will guarantee* these thermometers through all the usual ranges.

Grand geodæsical Operations in India.

It will be gratifying to the lovers of science to learn that a trigonometrical survey of India is carrying on by the munificence of the East India Company, and is to proceed with increased rapidity. No less than fifty-six theodolites have been prepared by one of the first makers in London, and sent out within the last two months. Major Everest, Surveyor-General of India, is also continuing the measurement in that country of an arc of the meridian of an enormous extent. This is using the "giant's power" for noble purposes!

Income of the Dublin and Kingstown Railway.

The first working year of this railway ended on the 16th of December last. The total receipts were 31,066*l.* 8*s.* 6*d.*; the number of passengers, exclusive of annual subscribers, 1,068,018; number of trips made by the loco-motive engines, 22,050; number of miles travelled, 125,275.

Two small Cabinet Figures in white Carrara Marble

Are at present deposited at the Adelaide-Street Gallery, with a view to sale. These *chefs d'œuvres* of modern sculpture formerly belonged to the unfortunate Queen of France, Marie Antoinette; they ornamented her saloon at Trianon, and were rescued from the destructive fury of the French revolution, by an Austrian nobleman, and carried to his palace near Vienna; where, during the Congress in 1814, they excited the admiration of the Allied Sovereigns. An offer of two thousand guineas was made for them by the late Marquis of Londonderry, then Lord Castlereagh, and Sir Thomas Lawrence, but this extravagant sum was refused by their owner, then at the very pinnacle of grandeur and prosperity. They remained in his possession until within the last few months; when from the most calamitous reverses of fortune, the consequence of a misplaced confidence in the present ex-King of France, he has been compelled to consign them to the care of a friend, whose only difficulty in disposing of them would, we apprehend, arise from the magnitude of the price asked,—fifteen hundred guineas.

The first represents Venus as having just risen out of the ocean, and in the attitude of wringing out her hair. Whether we consider the graceful and expressive disposition of the *tout ensemble*, or, wishing to fix on any one part as more worthy of admiration than another, select the contour of the back, and the remarkable truth and elegance displayed in the action of the arms and hands,—we are bound to admit, it presents so close and classical a resemblance to the antique as might deceive the most skilful artist, but that the features are those of the amiable Madame Elizabeth of France, sister to Louis the Sixteenth. The other figure is also that of a Venus seated in a large conch shell, and gliding as it were along the surface of the waters; she is drawn by two Tritons and supported by Cupids; the countenance of this figure is a portrait of the fascinating Queen herself, sculptured at the very time when, as Burke says "never lighted on this orb, which she hardly seemed to touch, a more delighftul vision." This, like the former statue, is devoid of drapery. The position of the body, and the disposition and action of the limbs, evince at once the most perfect anatomical proportion, and the most easy and graceful arrangement. The elaborate workmanship displayed in the minuter parts and accessories is highly deserving of attention in both the sculptures; but, perhaps, in the last mentioned the most indisputable evidence of the hand of the master is to be seen in the exquisite manner in which the bending of the body is effected.

METEOROLOGICAL JOURNAL FOR DECEMBER, 1835; KEPT AT BLACKHEATH ROAD.

	Barometer. 9 A.M.	Barometer. 3 P.M.	Thermometer. Min.	Thermometer. Max.	Solar Var.	Daily Temp.	Rad.	Clouds. 0—10			Wind. 0—6		Wind. A.M.	Wind. P.M.	Lunation.	WEATHER, &c.
Tuesday, 1	29·500	29·501	45°1	51°4	6·3	48°2	43°	2	1	7	1	2	S.	S.		Temperate; evening and night cloudy and squally.
Wednes. 2	29·585	29·699	42·5	48·0	5·5	45·3	41	1	1	1	2	1	S.W.	S.W.		Fine clear atmosphere.
Thurs. 3	29·801	29·800	38·0	50·1	12·1	44·1	34	4	4	6	1	2	S.	S.W.S.		Cloudy A.M.; much loose *scud.*
Friday, 4	29·881	29·905	39·5	46·0	6·5	43·8	37	1	5	0	2	1	S.W.	W.S.W.	○	Tem. A.M.; a shower at 3 P.M.; air cooler; clear night.
Satur. 5	30·301	30·351	36·0	45·1	9·1	41·5	33	5	6	5	1	1	W.S.W.	S.W.		*Cirro-cum.* into *cirro-stratus;* much precipitation.
SUNDAY, 6	30·300	30·252	37·1	44·8	7·7	41·0	34	4	2	8	0	0	S.E.	E.		Temperate; *cirro-stratus;* resolving into a fog.
Monday, 7	30·275	30·251	31·7	40·3	8·6	36·0	30	10	10	10	0	0	E.	E.		Cloudy throughout; air calm and close.
Tuesday, 8	30·214	30·050	35.5	40·5	5·0	38·0	35	10	10	9	0	1	S.W.	W.N.W.		Overcast; rain at night.
Wednes. 9	29·885	30·080	37·0	41·9	4·9	39·6	37	8	8	1	2	2	N. b E.	N.N.E		Windy; flying clouds and cold; clear at night.
Thurs. 10	30·425	30·485	26·9	34·1	7·2	30·5	25	10	5	0	0	1	S.E.	S.E.		Small rain A.M.; clear frosty night.
Friday, 11	30·374	30·370	21·0	30·9	9·9	26·0	18	0	0	1	0	1	E.	S.		Frost severe; air clear.
Satur. 12	30·405	30·400	20·7	37·0	16·3	28.8	18	9	9	1	0	0	S.W.	W.	☾	Cloudy and lowering; *cirro-cum.* and *cirro-stratus.*
SUN. 13	30·450	30·450	28·9	38·4	9·5	33.7	28	1	1	10	1	0	S.W.	S.		Clear; night cloudy and misty.
Mon. 14	30·475	30·450	28·7	40·7	12·0	34·7	27	10	10	10	0	0	Variable.			Thick, rather misty.
Tues. 15	30·459	30·450	35·0	39·1	4·1	37·1	34	10	10	10	0	0	W.	W.		Ditto, ditto.
Wed. 16	30·486	30·485	33·4	41·4	8·0	37·4	33	10	7	10	0	0	—	S.E.		Close, with drops of rain; damp and foggy.
Thurs. 17	30·490	30·424	33·0	39·2	6·2	36·1	32	10	7	10	1	1	S.W.	W.		Ditto, misty (*stratus*); cloudy; air getting damp.
Friday, 18	30·215	30·140	35·1	45·0	9·9	40·1	33	7	4	9	2	2	N.W.	N.N.W.		A violent squall about noon; cloudy.
Satur. 19	30·095	30·060	32·0	35·5	3·5	33·7	30	5	6	10	2	3·4	N. b E.	N.N.E.		Cold wind; air tends to dryness; a heavy gale at night.
SUN. 20	30·091	30·101	30·2	32·8	2·6	31·5	30	10	10	10	3	3	N.E.	N.E.	●	Snow fell in the night; strong wind with small granular [snow.
Mon. 21	30·250	30·300	30·0	32·2	2·2	31·1	29	10	1	0	3	2	N.E.	N.E.		Cloudy till noon; P.M. fine and clear.
Tues. 22	30·491	30·550	23·2	34·9	11·7	29·0	20	1	4	1	1	1	N.E.	N.E.		Frost, with thick ice; cloudy with *scud.*
Wed. 23	30·670	30·650	20·1	30·0	9·9	25·5	17	10	10	10	1	1	W.	W.		Ditto, ditto; overcast and dark with stratus and fog.
Thurs. 24	30·570	30·550	27·7	29·7	2·0	28·7	27	10	10	10	1	0	W.	W.		Ditto, ditto; dismally dark and misty.
Friday, 25	30·501	30·465	18·9	23·6	4·7	21·2	18½	10	—	10	0	0	W.	S.		Ditto; much rime; sun just visible through a dense red [mist.
Satur. 26	30·495	30·460	18·7	26·0	7·3	22·4	16	—	10	0	0	1	W.	S.		Ditto; clear at noon; cloudiness came on in the evening.
SUN. 27	30·401	30·380	19·0	37·0	18·0	28·0	15	8	10	10	1	2	S.W.	S.W.	☽	Cloudy; a thaw; *cirro-cumulus.*
Mon. 28	30·252	30·195	33·8	46·7	12·9	40·3	31	10	10	5	3	3	S.W.	S.W.		Wind high; *scud,* with a high temperature.
Tues. 29	30·358	30·375	35·1	41·8	6·7	38·4	30	7	9	10	2	1	W.	W.		Overcast and mild.
Wed. 30	30·340	30·335	38·5	45·5	7·0	42·0	36	10	10	5	2	2	N.W.	N.N.E.		Ditto; a squall from N. about 3 P.M.; air getting cold.
Thurs. 31	30·450	30·440	26·2	30·0	3·8	29·1	23	2	—	10	1	0	S.	W.		Hoar-frost; *cirro-cum.;* P.M. dense fog and frost.
Mean	30·268	30·266	31·08	39·18	7·82	35·13										

Max. of Pressure, 30·680 the 23d. | Mean at 9 A.M. 30·268 || Max. of Temp. 51·4 the 1st. | Mean Temperature 35·13 || Solar Variation 7°82.
Min. of do. 29·440 the 1st. | Do. at 3 P.M. 30·266 || Min. of do. 18·7 the 26th. | Min. of the Rad. 15° the 27th. || Rain fallen 0·49 inches.

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RECENT RESEARCHES IN GEOLOGY.

Geology is a subject of immense extent; and the discoveries which are made in it almost necessarily proceed by slow steps and minute details. Hence it would be utterly impossible, even in a memoir of considerable length, to give a complete and comprehensive survey of the recent progress of this rapidly advancing Science. But here, as in all sound inductive researches, the accumulation of particular facts generally terminates, after a while, in the developement of some great general principles. When such epochs occur, it is often very practicable to condense into a short compass, and in a generally intelligible form, a statement of the results so obtained. This is what we shall attempt, in the following article, with respect to one or two leading subjects of geological inquiry, which have not only excited peculiar interest of late, but also have important bearings on the principles of the Science, and on some of the most instructive inferences and contemplations into which we are led by the study of it.

The conclusions of geology, like those of every other part of inductive science, must be grounded on the *sole* authority of well-ascertained and classified facts; and we must be guided to them, neither by random conjectures, nor the dictation of authoritative opinion, but by the *sole* pursuit of well-founded natural analogies. We must seek to interpret the past from the present, and advance from the known to the unknown.

Proceeding on such principles, then, we shall presume that our readers will acknowledge the force of the reasoning by which it is inferred that where two beds, or strata, lie one over the other, the former was deposited or formed *subsequently* to the latter; that each one of the vast number of lesser beds or layers, of which even a small thickness of any stratum is composed, were all formed one after another; and, when we come to distinguish the larger divisions and classes of strata, by the fossil remains of plants and animals, which we find embedded, and often completely mineralized in them,—that these are the remains of creatures which actually lived and died during the period at which the depositions took place respectively; and that the lowest rational estimate we can form will not allow us to suppose any short or limited period of time as requisite for the formation of any one bed, the enclosing in it of all its organic remains,, and (marine or aquatic as those remains so universally are) its elevation from the bottom of the primæval ocean into dry land.

Pursuing our researches on these simple and truly philosophic principles, we are brought in succession to recognise an immense series of deposits, characterized by organic remains, in which the skill of the naturalist and the anatomist detects species, genera, entire orders of living beings which do not now exist. The deposits in which these occur, now in a great degree hardened and consolidated into rocks, are thus characterized as distinct formations, which have gradually emerged at successive remote epochs, at incalculably long intervals of time. Other classes of phenomena are observable in a series of rocks of a different texture, and wholly destitute of organic remains, which appear protruded, as it were, among and through the others: having, in many cases, an exact resemblance to the effects of existing volcanoes,—and in all, following a close analogy to such modes of erruptive action.

We shall in the following sketch presume no further on our reader's acquaintance with the subject than to the extent here briefly described. The names given to the successive leading groups of formations, which all over the world succeed one another in this order, are principally, the tertiary (or newest), above the chalk. The secondary, from the chalk inclusive to the coal formations: then those which have been called transition; and, lastly, the primary, of crystalline texture, without organic remains, and bearing marks of being upheaved, protruded, or forced through all the others, in the way that masses of melted matter are now forced up by volcanic action.

Silurian and Cambrian Formations.

Mr. Murchison and Professor Sedgwick have been for a long time directing their joint labours to the elucidation of the rocks usually confounded together under the unmeaning name of "Transition," comprising all the series intervening between the old red sandstone and the primitive rocks. They have been minutely examined by these two eminent geologists, as developed in Wales and the part of England adjoining, and they have succeeded in dispelling almost entirely the obscurity in which the nature of these rocks has been long involved.

From beneath the old red sandstone, there rises out this considerable group of rocks which, taking them in the order from upper to lower, Mr. Murchison has named the Ludlow, Wenlock, Caradoc, and Llandeillo formations, each being distinguished by characteristic organic remains, and frequently by subordinate beds of limestone. These beds form a well-marked connected group, interposed between the old red sandstone and the slaty-grauwacke of Wales. Hence it seemed very desirable to apply some distinctive name. So great have been the recent advances in geology, that the term "transition," formerly applied, has (as we observed above) now become wholly unmeaning, and, in fact, conveys incorrect impressions. Hence these geologists have adopted the name "Silurian System," (from the Roman name for this part of Wales;) and this they subdivide into the "Upper Silurian," comprising the two first of the four classes named above,—and the "Lower," including the two last.

Beneath these appear the various slaty rocks, which are common to Wales and Cumberland. These Professor Sedgwick has minutely investigated; and divides them, according to order of superposition, into upper, middle, and lower. The upper formation is seen in the chain of the Berwyn mountains, and is thence expanded over a large part of South Wales, including Plinlimmon, it contains in general less calcareous matter, and fewer organic remains, than the Silurian systems. The middle Cambrian includes the Merionethshire ranges and Snowdon, containing a few organic remains, and some highly calcareous slates, but no beds of limestone. The same group is largely developed in Cumberland. The lower or oldest Cambrian group occupies the south-west of Caernarvonshire, and much of Anglesea. It contains no organic remains.

In this rapid sketch, we, of course, can do little more than explain the nominal distinctions which have been thus laid down. But it must be understood, that they are far from being mere distinctions of names. They involve essential characteristics of extensive geological districts, and serve to bring under a luminous classification a series, of great importance tc a connected knowledge of British strata, which has long been involved in obscurity from want of such a principle of arrangement.

A full account of these researches was given at the Dublin meeting of the British Association; and elicited, besides the encomiums so justly due to the talents and perseverance of its authors, many able illustrations and remarks; especially from Mr. Greenough, who considered that similar principles of classification might very probably be extended to other regions; and from Professor Phillips, who made some highly interesting observations on the distribution of characteristic organic remains in rocks, especially those here considered. He dwelt upon the important fact, so utterly destructive of the favourite hypothesis of some geologists and cosmogonists, of a gradual advance from the simplest to the most complex forms of animal life, as we advance to the newer rocks; that in these Silurian groups, though we find a diminution in the number of fossil species in the older rocks, yet they exhibit *no inferiority of structure or organization.* They belong to extinct classes. Among beings of lower organization, as among shell-fish, some single species may be found even in rocks so ancient as the Silurian system, which also now exist; and he was hence led to remark, that it is not by any single genus, but by a combination of co-existing genera that strata must be identified.

Fossil Fishes.

The natural history of fishes has been generally considered more obscure than that of any other of the great divisions of the animal kingdom; and it has been almost entirely through the labours of M. Agassiz that a new light has been thrown over it, by tracing out, as he has done, a new principle of classification: by this the whole science has been remodelled. It is also a singular circumstance in this investigation, that (contrary to the usual order of procedure) the study of the fossil remains of fishes has been a material source of elucidation for understanding the relations and classification of existing species.

The great principle of classification adopted by M. Agassiz, is derived from the nature of the *external covering, or scales.* The peculiar form and structure of the scales differs essentially in different classes of fishes; and the nature of the covering, which protects the animal externally, is found to bear a direct relation to the internal organization. Here then there appears a principle of relation which, doubtless, depends upon some essential modification of the animal character, and thus may fairly afford a satisfactory ground of a real distinction and classification of species. This principle, then, M. Agassiz has adopted; and, in following it out, has arrived at a grand distinction of fishes, under four principal orders, characterized by the peculiar nature of their scales. They are termed, 1. Placoïdians, 2. Ganoïdians, 3. Ctenoïdians, and 4. Cycloïdians[*].

Of the whole number of species now known to exist, more than three-fourths belong to the two orders of Cycloïdians and Ctenoïdians, the other fourth to the remaining two. Whereas, of the species whose fossil remains we find imbedded and mineralized, none of the two orders last-named have been found in any formations below the chalk; whilst in the lower or older formations we have abundance of the other two kinds. The proportions of these in the different formations are very remarkable, and have been carefully traced by the persevering industry and skill of M. Agassiz.

In the most recent or tertiary deposits, not only the fossil orders and genera, but also the species, *approach* nearly in character to those now existing: though he has not found more than one *species exactly* the same. Those of the formation called "crag," in Norfolk, are allied to the species now inhabiting the tropical seas. In the London clay, the beds in the basin of Paris, and at Monte Bolca, about two-thirds belong to existing genera.

In the formation next below these, the chalk, about one-third only belong to existing *genera*.

In the formations older than the chalk, there is not a single *genus* identical with the recent. The oolitic series, to the lias inclusive, forms by its species of fossil fish a very natural and well-defined group. The weald formation is included in this, in which M. Agassiz did not find a single species referrible even to the genera of the chalk.

Throughout the series of rocks deposited in these epochs, the two *orders* which prevail by so large a majority (as above stated) in the existing creation, are not to be found. New species have since been created; the whole genera formed of all those species are new, not merely in a few instances, but through such a range and extent, that even the entire *order* comprising those *genera* is new. In these older strata, on the other hand, different species, genera, and orders, existed in proportional abundance, most of which have since died away and disappeared; and the two great orders which at the present day form a small minority, were then predominant.

The most striking characteristics, perhaps, of these periods are the predominance of those Ganoïdians which have a symmetrical caudal-fin; and those Placoïdians, which have their teeth furrowed on both sides, and have large thorny rays on the dorsal-fin. These fossil rays had long been known, but their real nature was wholly misunderstood.

In the formations below the lias, the character, above stated, in the tail-fin of the Ganoïdians is entirely changed. Instead of a tail parting off into two equal and similar divisions or lobes, the backbone is continued straight on, into a true tail, while another lobe, or fin, is formed beneath, so as to give the appearance of a tail-fin, with two unequal, unsymmetrical, lobes. This distinction prevails up to the fishes of the most ancient strata.

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[*] These names are derived from Greek words, describing the shape and appearance of the scales.

1. From πλαξ, a table or broad surface, the scales being large.
2. From γανος, beauty or splendour; from the bright enamel with which they are armed.
3. From κτεις, a comb, the scales being formed with teeth.
4. From κυκλος, a circle, the scales being round.

The form of the teeth is another important distinction, bearing obviously a direct relation to the habits of the animal and its means of subsistence.

In strata more recent than those containing coal, we find no fish decidedly carnivorous,—that is, provided with large conical and pointed teeth. In these strata, up to the chalk, the fish appear to have been omnivorous, their teeth being either rounded, or in obtuse cones, or like a brush. The nature of the food of these fish is also ascertained by the discovery of the fossil contents of their intestines, in which scales of other fish, on which they had preyed, have been found.

In a great number of instances from the tertiary beds of the Isle of Sheppy, the chalk, and the oolite rocks, it is a highly interesting fact, that the capsule of *the eye* has been preserved: and in many species from Monte Bolca, Solenhofen, and the lias, we see distinctly all the little blades which form the branchiæ.

In the strata below the lias, we begin to find the largest of those large fish, of an organization allied to the Saurian, or lizard tribe; the resemblance is chiefly in the mode of connexion of certain parts of the skeleton, and the form of the teeth.

From the general distribution of the species of fossil fish thus investigated, M. Agassiz has deduced some important and profound inferences, with regard to the changes which our planet has undergone at remote epochs.

There is a remarkable distinction between these fossil fishes, and the fossil *zoophytes and testacea.* Of these last, the same genera are found through several different formations, as we have already noticed; and their organization was such as enabled them to live through all the great changes in the physical condition of the globe, which accompanied the successive depositions of those formations. With the fossil *fishes* the case is widely different. We have seen that the several genera, and even orders, vary extremely from one formation to another. Thus the changes in the constitution of the globe, which accompanied the successive epochs, were of such a kind as these genera of fishes were unable to survive. We see at once, then, a reason for this difference between them and the inferior classes, in the greater perfection and delicacy of their organization; their more complicated structure required important modifications, according as great changes took place in the climate, and various physical relations in the order of things on the surface of the globe. Here then was the same beautiful series of adaptations, existing in as high perfection myriads of ages ago as at the present time: displayed equally in all the long series of creations, by which the globe has been gradually brought into its present condition, and evincing the ever-enduring and universal influence of the same creative Power and Intelligence.

The fishes *of each of the great* periods of the earth's formation are thus essentially different from each other, but each series agreeing among themselves in some peculiarities of organization. The multitude of species so coexisting must, doubtless, have been fitted by that peculiar organization for the particular conditions which prevailed on the surface of the globe at the time they lived. So, likewise, the disappearance of whole species and genera, is the evidence of great and universal changes in those attendant conditions of the external world, which introduced a new order of things unsuited to that peculiar organization with which they were furnished. Thus, not only individuals, but whole families and species perished: not only a few species, but a wide range of species, comprising a whole genus; and not only this, but so many genera as made up the larger portion of an entire order. One common peculiarity constituted the distinction of the order; that peculiarity was no longer suited to external nature,—the whole order therefore perished.

Did then these vast alterations in the plan of nature take place suddenly? Was this immense destruction, not only of animal life but of a whole system of organization brought about at one time?—in a short time; or did it take place by more gradual changes? by a series of changes so slow as to be imperceptible, going on through a countless series of ages?

M. Agassiz has introduced some remarks bearing on the solution of these questions. He observes that, in some cases, local and transient causes may be capable of producing such effects over a certain extent of district: such, for example, as volcanic eruptions. A submarine eruption might destroy all the fish in the particular region where it took place; but this would hardly account for the disappearance of *species* and *genera*, however extensive or often repeated. M. Agassiz possesses specimens in which a great number of fossil fishes are crowded into a small space; and the appearance of the whole is such, as to impress the spectator with the belief that they were destroyed and imbedded, as it were instantaneously, by some sudden catastrophe: such as a sudden eruption of volcanic matter, or a sudden influx of fresh water, or even the heating of the sea by a submarine volcano.

Such causes as these, however, could be only local; and it is evident we must refer to changes, upon a much larger scale, in the condition of the earth's surface, as alone capable of producing the greater effects we have above described. M. Agassiz appears to lean to the theory of those geologists, who contend that the great changes which have affected the crust of our globe were brought about by vast and sudden catastrophes; and that, corresponding with the occurrence of these convulsive movements, the great changes in the characteristics of animal and vegetable life were as suddenly introduced. Those of our readers who have perused the masterly but extremely popular work of Mr. Lyell (and we hope there will be few who have not), will know how to estimate the claims of this theory.

M. Agassiz commenced his researches on the Continent; but has, more recently, extended them to an examination of the specimens found in English collections. Here, indeed, we have been long accumulating these geological treasures, in which our island is peculiarly rich, but without fully understanding the value of them, until M. Agassiz has pointed it out to us; and invested many of these accumulations of neglected remains, with a new value and interest. He has found, in the English cabinets, 300 species *new* to his researches. Here, then, was an interesting, perhaps critical, moment for his speculations: his views were thus put to a severe test. All these specimens, however, were found to furnish a complete verification of his former inferences, and entirely to corroborate the laws of developement which he had previously determined, in regard to the succession of these orders of animals, during the different changes which our globe has undergone.

The oldest formation in which fossil fishes are found, is the Silurian system of rocks; in which there are five or six species, exhibiting the first appearance, in the primæval world, of this long-continued series of vertebrated animals; the species of which become more and more diversified and numerous, as well in their outward forms as in their organization, as we advance to the later formations. Yet, as we have indeed already noticed, those which do occur even in these most ancient rocks, the first of living beings which tenanted the globe, were animals of the most perfect and exquisite structure and organization.

A highly interesting account has lately been given to the Geological Society, by Mr. Murchison, of the discovery of fossil fish, in the new red sandstone of Tyrone, in Ireland, being the first discovery of such remains in that particular stratum, though they were known to exist in others of the group to which it belongs. The part of the formation in question surrounds, and includes, a small coal-field, but reposes, for the greater part, on mountain limestone. The sandstone consists of many distinct beds, which have evidently been deposited at different, and widely separated, periods of time; since some of the lower exhibit, on the upper surface, the marks of the rippling action of water, and must, therefore, have long presented an exposed surface to a calm sea. It is in the lowest beds, twenty-five or thirty feet below the surface, that the fishes are found.

Another curious and interesting fact, connected with these researches, has been the light thrown by them upon some very singular specimens, which had been, for years, in the possession of Dr. Buckland, but of which neither he, nor any of the numerous geologists and naturalists who examined them, could make out anything. They are now ascertained, by comparison, to be the jaw-bones of a rare fossil fish, of which four different species are now recognised, in the oolitic formation, by the acute investigations of Dr. Buckland.

We have alluded to some remains of fossil fishes, which bear a resemblance to those of the Saurian reptiles, (the tribe including the lizards and crocodiles). We must not omit one very remarkable instance, which throws considerable light on this sort of relation.

In the limestone of Burdie-house, in Scotland (belonging to a deep-seated bed of the coal formation, beneath all the beds of coal), numerous specimens of fossil fish were discovered by Dr. Hibbert, a few years ago. Amongst other remains, he has since found, in this locality, many specimens of teeth, scales, and large bones, apparently of a Saurian character. This being mentioned to M. Agassiz, he at once traced an analogy which has enabled him to explain, with the highest probability, the nature of the animal to which *they belonged*. In the tropical parts of America, he immediately called to mind a species of fish (the Lepidosteus), now existing, which has, in many points, a striking resemblance to the lizard tribe. The form of its scales particularly, as well as of its teeth, are extremely similar to those of the crocodile genus; and even its internal organization forms a sort of connecting link between that of a fish and a lizard. The swimming-bladder, when minutely examined, is found to be, anatomically, a true *lung;* and approaches closely in structure to the lungs of reptiles. It has a regular trachea, communicating with a glottis, surrounded by ligaments intended to open and shut it, constituting an apparatus even of a more complicated structure than that of many reptiles. The heart, again, resembles that of a reptile in some particulars.

With this fish M. Agassiz compared the sauroid remains (as they had been provisionally termed,) found at Burdie-house: he was materially assisted, also, in making out the analogy, by the entire head of a large fossil fish in the museum at Leeds. By this sort of comparison, he has at length classified the fossil remains into a new genus, under the name of Megalicthys; of which more than one species are now recognised in the coal-fields of Scotland.

These investigations were given in a paper, read to the Royal Society of Edinburgh, by Dr. Hibbert (Dec. 1834), who, in conclusion, well observed the importance, in geology, of such analogies with living species. In this instance, he observed, "M. Agassiz had rescued from obscurity a sauroid fish, dwelling among the lakes and rivers of the most thermal regions of America, and rendered it elucidative of one of the earliest states of our planet, when, in the language of this naturalist, fish united, in their particular organization, the character of reptiles, belonging to that class of animals which only appeared in far greater numbers during a later epoch.

Elevation and Subsidence of Land.

A paper was read to the Geological Society, November 18, 1835, in which Dr. Pingel, of Copenhagen, gives a detailed description of the evidences which he has collected in a tour, of the fact of a *subsidence*, during the last half century, of the *west coast* of Greenland, between N. lat. 60° and 69°. This is evinced by the ruins of houses and villages on the shore, which are now covered at high water, and, in some instances, are only visible at very low tides.

From a statement, by Captain Fitzroy, R.N., read at the same meeting, it appears that the earthquake of February, 1835, on the coast of Chili, not only produced an alteration in the currents, but that the island of St. Maria was permanently elevated ten feet. Another, and more detailed, account of the same earthquake, was given in a letter from Mr. Alison. The most remarkable circumstances were these: Forty minutes after the first shock, the sea suddenly retired so far, that a great part of the bottom of the bay, at the port of Talcahuano, was laid dry; but the water very soon afterwards returned with increased violence, and flowed twenty feet over the town, carrying everything before it. This was repeated three times. The same thing occurred in the earthquake which destroyed Penco, in 1730 and 1731. In the present instance, the land permanently rose two or three feet on the shore and in the bay. Mr.

Alison also mentions the existence, near Valparaiso, of *recent* marine shells, 1400 feet above the level of the sea; and, in the bay of Valparaiso, he says, a rock, which, in 1817, could be passed over in a boat, is now dry except at spring-tides.

At the island of Juan Fernandez, a similar recession, and then violent influx of the sea took place; but here it was accompanied by another remarkable phenomenon, namely, the breaking out of a submarine volcano, which caused an immense agitation and boiling of the sea.

At a meeting, on December 2nd, a paper was read on the effects of earthquake waves on the coasts of the Pacific, by Woodbine Parish, Esq. In this valuable memoir, the author has collected all the information he could obtain from well-authenticated historical accounts of earthquakes, producing those sudden overflowings, or rather immense waves, in the sea of which we have just spoken. These are highly curious, and attest the vast force with which inroads of the ocean, under these circumstances, have taken place. They by no means *always* accompany earthquakes: in fact it evidently depends on the direction which the shock takes, and the locality of its origin, whether such an effect will be produced or not.

A most remarkable and instructive example of the gradual and permanent rise of a large tract of land, is that described by Mr. Lyell as now taking place in Sweden. (*Phil. Trans.*, 1835, i.)

It is more than a hundred years since the Swedish naturalist, Celsius, declared his opinion that the level of the waters, both of the Baltic and the ocean, was suffering a gradual depression; that is, that there was a change in the *relative* level of the land and the sea.

Von Buch, in the course of his tour in Sweden and Norway, about twenty-five years ago, found, at several places on the western shores of Scandinavia, deposits of sand and mud, containing numerous shells referrible to species now living in the neighbouring ocean. From this circumstance, and from accounts which he received from inhabitants of the coasts of the Bothnian Gulf, he inferred that Celsius was correct in regard to a gradual change of relative level. As the sea cannot sink in one place without falling everywhere, Von Buch concluded that certain parts of Sweden and Finland were slowly and insensibly *rising*.

Some difference of opinion, however, prevailed on the subject; and there were not wanting able observers, who contended that the inferences were founded on mistaken data, and, on other grounds, denied the truth of the statement altogether. The question thus acquired additional interest: and Mr. Lyell, determined to investigate the facts himself, made a tour into Sweden for the purpose.

He visited a considerable part of the shores of the Bothnian Gulf, between Stockholm and Gefle, and of the western coast of Sweden, between Uddevola and Gothenburg, districts particularly alluded to by Celsius. He examined several of the marks cut by the Swedish pilots, under the direction of the Swedish Academy of Sciences, in 1820, and found the level of the Baltic, in calm weather, several inches below the marks. He also found the level of the waters several feet below marks made seventy or a hundred years before. He obtained similar results on the side of the ocean; and found, in both districts, that the testimony of the inhabitants exactly agreed with that of their ancestors, recorded by

Celsius. After confirming the accounts given by Von Buch, of the occurrence, on the side of the ocean, of elevated beds of *recent* shells at various heights, from 10 to 200 feet, Mr. Lyell discovered, in addition, deposits on the side of the Bothnian Gulf, between Stockholm and Gefle, containing fossil shells of the same species which now characterize the brackish waters of that sea. These occur at various elevations, of from 1 to 100 feet, and sometimes reach fifty miles inland. The shells are partly marine and partly fluviatile: the marine species are identical with those now living in the ocean, but are dwarfish in size, and never attain the average dimensions of those which live in waters sufficiently salt to enable them to reach their full developement. Mr. Lyell concludes, in general, that *certain parts of Sweden are undergoing a gradual rise, to the amount of two or three feet in a century*, while other parts, visited by him, further to the south, appear to experience no movement.

All these facts have an extremely valuable bearing on geological theories. The fact that a slow, and imperceptibly gradual rise, is now taking place in one large district of the earth, whilst a sinking has been also going on in another, afford us the strong ground of observed facts for admitting, as a true philosophical cause, the like slow, gradual, elevation of land out of the sea, in other cases and in earlier epochs. To account for the actual appearances of strata, now hundreds or thousands of feet above the sea, containing beds of marine shells and animal remains, we require nothing but a repetition, or rather constant succession, of such events as are now going on in Sweden and Greenland, to account for all those level, or but slightly inclined depositions, which, to so vast an extent, have contributed to form our existing continents. And where is the slightest ground of probability for supposing these changes to have gone on at a more rapid rate formerly than now? And if we form anything like the roughest calculation, what length of time shall we assign for the elevation of any one, even of the superficial and most recent formations?

But the effects of earthquake waves is a subject not less worthy of consideration. Some geologists have contended that the sudden elevation of mountain chains, by volcanic forces, of whose intensity nothing, in the present degenerate condition of the globe, can convey any idea, caused mighty waves, deluging, at one sweep, vast regions of the earth, and accounting for numerous phenomena, which those of another school attribute to the action of ordinary causes, acting through immensely long periods of time. These earthquake waves give us an idea of the extent to which such causes can act under the influence of volcanic forces, of the highest intensity of any within human experience; *i. e.*, capable of producing local inroads, on particular coasts, to an extent absolutely insensible compared with those which must be imagined in order to account *in this way* for geological phenomena. We may then calculate, in some degree, what enormous intensity must be supposed in an earthquake, to cause an inundation of any considerable tract of country. Further, we must own, we find it impossible to conceive how the upheaving of a mountain, from the bottom of the sea, supposing it merely to ascend uniformly and steadily, could produce *any wave at all.* It seems to us solely the trembling motion and rapid shock of the earthquake, which produces the wave.

FLAMSTEED, NEWTON, AND HALLEY.

THE names of these great contemporary luminaries of British Science are familiar to all: but of the first, except in general that he was the father and founder of practical Astronomy in this country, very little has been hitherto known. His labours, indeed, the Catalogue of the Stars, and other observations contained in the *Historia Cœlestis*, have been long celebrated; but of the history and character of their author, or the very singular circumstances of their publication, hardly anything had been known until within the last few years, when that able and zealous astronomer, Mr. Baily, not only searched out a mass of original records, in the library of the Royal Observatory at Greenwich, including Flamsteed's private diaries, but also fortunately discovered an extensive correspondence between Flamsteed and his friends and coadjutors, Messrs. Sharp and Crossthwait. On these materials, Mr. Baily has founded an able memoir, which, together with the original documents, a corrected edition of the Catalogue, and some other subsidiary matter, have been, under his superintendence, printed at the public expense, by direction of the Lords of the Admiralty, in a handsome quarto volume; all the copies of which have been gratuitously distributed among scientific bodies and individuals.

The volume is replete with interest; not merely to the astronomer, but to every one for whom the history of Science possesses any attractions; and, indeed, to all who delight in studying the human character in its strength and its weakness. The subject, as might be expected, has attracted much notice.

The narrative disclosed by Mr. Baily, exhibits Flamsteed in continued intercourse with Newton and Halley. But under such circumstances as, according to the testimony here adduced, tend to place the characters of the two last in an extremely unfavourable light; in fact, to cast upon them imputations of motives of the most discreditable, and conduct of the most dishonest kind. At least, as far as Newton is concerned, these charges are as new as they are painful to contemplate, since from all representations hitherto given, his character has always been esteemed one of even unusual moral excellence. Hence, the statements of Flamsteed have been received with mingled surprise and regret.

In the *Quarterly Review*, No. CIX., an article appeared on the subject, which we must confess we read with an almost unmixed feeling of disappointment, at the total want of anything like a comprehensive discussion or elucidation of so interesting a question.

In addition to the public interest which everything relating to Newton must inspire, it was to be expected that a strong feeling would prevail on the subject in his own university; and, accordingly, Mr. Whewell has taken up the question in a very able though short pamphlet, in which he enters upon Newton's vindication, especially against the Quarterly reviewer, with equal skill and candour. In the succeeding number of the *Quarterly* a note was inserted reflecting on Mr. Whewell's observations; to which that gentleman has printed a reply in the *Cambridge Chronicle* of February 5th*.

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* Mr. Whewell *has since reprinted* his pamphlet with this letter appended, *together with another referring* to a separate point of accusation.

An article in the *Edinburgh Review*, No. CXXVI., is in every way of a much higher stamp. But the writer, though much more able in his discrimination of character than his contemporary, yet to our apprehension leaves much to be done in the explanation of the conduct of the respective parties, on something like reasonable ground.

The narrative of their proceedings and differences, their violent animosities and protracted hostilities, is not a mere history of personal squabbles, but is deeply interesting as connected with the characters of the most eminent men of science whom this country has produced; and embraces an important portion of the history of astronomy. Mr. Baily has conducted his valuable publication in a luminous, scientific, and straightforward manner: but he enters little into any discussion tending to clear up the difficulties of the case, or to explain the singular position in which we find the parties: it was, in fact, no part of his design to do so; he discharges the duty merely of the unbiassed editor; who, indeed, brings to light a vast number of new documents and facts, the tendency of which is to impugn the character of highly eminent persons; but he appears neither in the capacity of accuser nor defender. The impression left by a perusal of the volume, as it consists of Flamsteed's own statements and those of his friends, is, of course, on his side. Our object will be, in the following remarks, to give a brief and impartial sketch of the outline of the history here presented to us, accompanied by such remarks as, upon a careful comparison of what has been urged on both sides, seem to us to afford at least some clue to the explanation of the circumstances.

John Flamsteed was born near Derby, August 19, 1646, and received the rudiments of his education at the Grammar-School in that town: he appears to have been of a sickly constitution, and during his youth had some severe attacks of illness. Throughout his life he was more or less a sufferer from constitutional complaints, which were certainly not alleviated by his intensely studious habits, nor by the laborious prosecution of astronomical observations in which he was occupied incessantly during the greatest portion of his life. This circumstance should be borne in mind as elucidating and palliating many instances of a corresponding infirmity of temper, which the narrative discloses.

While quite a youth, Flamsteed displayed a decided turn for mathematical and astronomical pursuits. He was wholly self-taught in these sciences, and followed up his inquiries as the amusement of hours of ill-health and seclusion. He formed acquaintance with some young men who followed similar pursuits, and composed mathematical tracts on dialling, trigonometry, and the equation of time. He also calculated tables of the places of the stars, on the basis of those of Tycho Brahe; and made an accurate observation of an eclipse of the sun. He soon advanced to the prediction of eclipses; and having calculated one, together with some occultations of stars by the moon, he communicated them to the Royal Society. The excellence of these investigations procured him considerable notice, and encouraging letters from Oldenburgh, the secretary of the society, and Collins, one of the first mathematicians of the day.

He shortly after formed a personal acquaintance with these and other eminent men in a visit to London, and especially with Sir Jonas Moore, (Surveyor of the Ordnance,) who continued through life one of his most valued friends. This gentleman presented him with some instruments, with which he afterwards continued observations at Derby: but on his return from London he visited Cambridge, where he became acquainted with Barrow and Newton; and entered himself at Jesus College.

We shall not follow him through the details of his astronomical labours, but merely observe that though we have abundant evidence of his limited attainments in the higher and more philosophic branches of science, yet in this particular department he seems to have perceived, as it were by intuition, precisely the right path by which the improvement of astronomy was to be effected: he appears at once to have seen the importance of a much higher degree of accuracy than was at that time commonly aimed at; and to have proceeded with equal skill and resolution to the practice of it in his observations. Yet, of the grand discoveries which soon after were made, even to the end of his life, he had very inadequate conceptions.

To proceed with the sketch of his history,—he kept his terms at Cambridge, without materially interfering with his observations at Derby, or his visits to the metropolis; and obtained the degree of M. A. in 1674. He was ordained soon after, and subsequently presented to the living of Burstow in Surrey. He was much with Sir J. Moore in his official residence in the Tower of London, where he made many observations. But the important event which determined his future course was the erection of the office of Astronomer Royal, the foundation of the Observatory at Greenwich, and his own appointment to it in 1675.

These measures originated in a proposal made by the Sieur de Saint Pierre, for finding the longitude at sea by means of the moon's distances from the fixed stars. In a report upon it laid before Charles the Second, Flamsteed pointed out that the method could not be used owing to the deficiency of the existing tables of the moon's motion. The king, startled at the assertion, said with some warmth, "he must have them anew observed, examined, and corrected for the use of his seamen." And on the necessity of accurate observations for this purpose being further intimated, he said, with the same earnestness, "he must have it done." On being asked who he would find to do it, with his usual readiness Charles at once replied, "the person who has pointed it out," and Flamsteed was forthwith named Royal Astronomer, with a salary of 100*l.* per annum, though with promises of necessary instruments and assistance. The building of the Observatory was soon completed; but when settled there Flamsteed began to complain that instruments were not supplied, nor the promised assistance granted him. In his letters and diaries he continually recurs to these grounds of complaint, and the expense he incurred for carrying on the observations. The only instruments he had were some given him by Sir Jonas Moore, and others constructed by himself and his assistant, Sharp, who was equally skilful as a mechanic and a calculator. Yet with the most praiseworthy spirit and zeal he persevered under all discouragements.

At first, he had only a sextant, by which the places of the stars could not be referred to any fixed point. It was not till 1684 that an instrument fixed in the plane of the meridian was erected, called the mural arc.

But before this was done, and when he consequently had no means of determining the *absolute* places of the stars, he was beset with importunities to print his observations. "Some people to make him uneasy, others out of a sincere desire to see the happy process of his studies, not understanding amid what hard circumstances he lived, called hard upon him to print his observations." He explained the reason of his being unable to do so by the facts just stated: he mentioned that he was engaged in preparing a complete catalogue of the stars. This only further excited the expectations of those who little understood the real extent of such a work. He writes (February 1691-2) in a complaining style to Newton, of the annoyance he received by this sort of importunity; and, indirectly, attributes the principal share of it to Halley. He alludes to Halley's having used a dictatorial tone towards him, which he resents in the strongest manner; retorts upon Halley in bitter sarcasms on the imperfections of *his* published observations; and hints, "he has more of mine in his hands already than he will either own or restore; and I have no esteem for a man who has lost his reputation, both for skill, candour, and ingenuity, by silly tricks, ingratitude, and foolish prate; and that I value not all, or any, of the shame of him and his infidel companions."

In 1694, at Newton's urgent request, Flamsteed gave him copies of a number of observations of the moon's places. He understood, that Newton's object was to complete the theory of the moon's motions as derived from gravitation: but seems to have had no notion of what that theory really amounted to. However he gave Newton the observations on two express stipulations; first, that he should not communicate them to any one without his consent; secondly, that he should be the first informed of any results which Newton might deduce from them.

The first condition, he says, Newton kept, but broke the second, "through insinuation, I fear, of some persons, that were little his friends till they saw what friends he had in the government." They continued to correspond on the subject, though Flamsteed, conceiving himself unfairly treated, as the correspondence proceeded, wrote in a tone of increasing coldness and even acrimony. Newton mentions (in October, 1694), that Halley had *desired* to see the lunar observations, which he *refused*, on the plea of his promise. And in a subsequent letter, in reply to a suspicion intimated by Flamsteed that he had shown them, he speaks in a tone of the most perfect mildness and conciliation, amounting to a dignified rebuke, to Flamsteed, for entertaining such suspicions, and acknowledging in the highest terms the value of his observations. He adds a request for some more results; these Flamsteed gave, to the number of one hundred more places of the moon, which (he takes care to add) "was more than he could reasonably expect from one in my circumstances of constant business and ill health;" enlarging upon all the circumstances in detail. Thus complaining of a request, yet complying with it, seems to have been his way of proceeding throughout. In the present instance, he evidently thought that in all this, he was merely doing a *personal* favour to Newton. "He ceased not to importune me (though he was informed of my illness,) for more observations; and with that earnestness, that looked as if he thought he had a right to command them; and had about fifty more imparted to him. But I did not think myself obliged to employ my pains to serve a person that was so inconsiderate as to presume he had a right to that, which was only a courtesy."—(p. 62.) This shows how little he was able to appreciate the value of these observations, or the use which Newton was making of them.

Newton, in one of his letters, reproaches Flamsteed, for his backwardness in supplying these observations, reminding him, that he had before furnished him with several valuable mathematical rules and deductions, and appealing to his gratitude. This, however, Flamsteed does not appear to have noticed. And shortly afterwards, a more open misunderstanding ensued between them, occasioned, in the first instance, by the apparently trifling circumstance, that Flamsteed, in a tract which he published (in 1698), had mentioned Newton's having derived lunar observations from him, by which to correct his theory. This Newton resented as an unauthorized publication of his name; and expressed in much warmer terms than the occasion would seem to call for, his dislike of "being printed on every occasion; much less to be dunned and teased by foreigners about mathematical things, or to be thought by our own people to be trifling away my time about them, when I should be about the king's business." Flamsteed's reply, though in a better temper, shows a complete misapprehension of the import of this expression of Newton: which obviously refers to the duties now incumbent on him as warden of the Mint, and the suspicions of the world that he neglected those duties, for what they regarded as the trifling pursuits of science.

Flamsteed had been one of those proposed for the office of mathematical tutor to the young Duke of Gloucester. Dr. Greogry was another. Flamsteed's jealousy was, therefore, now called forth against Gregory; and he suspects him of instigating Newton against him, in the design of ingratiating himself with Newton, through whose favour and interest he hoped to get the appointment. In addition to this, he was still full of similar suspicions against Halley, for (in 1700), he writes to his friend Lowthorp, (speaking of Newton,) "he is so possessed with prejudices against me by *some people's* suggestions, whom you know well, that I can have no free discourse with him." And again, "I believe him to be a good man at the bottom, but through his natural temper, suspicious, and too easy to be possessed with calumnies, especially such as are impressed with raillery." There was, probably, no little truth in this view of Newton's character.

Meanwhile, Flamsteed continued, with unabated zeal and perseverance, his observations on which the catalogue of the stars was to be founded. In his memorials, he never fails to enlarge upon the labours and hardships he had to encounter: and they were doubtless great; but from this incessant recurrence to them, we cannot fail to perceive that they were not a little aggravated by the temper of the individual. He also betrays a spirit of invidious jealousy against Newton; of whom he complains, that "as he was advanced in place, so he raised himself in his conversation and became more magisterial." Yet we find, that in 1704, coming to the observatory, and seeing what had been done towards the *completion* of the catalogue, he proposed to recommend it to the patronage of Prince George of Denmark, privately. This Flamsteed refused, suspecting a trick, though it does not distinctly appear what. After this, he says, he heard no more of Newton's recommendations. But "his (Newton's) flatterers, and such small mathematicians about London as hoped to get themselves esteemed very skilful, by crying up his book, began to ask, Why I did not print? as if I were obliged to publish my works just when they pleased," &c. However, Flamsteed was so far moved by the representations which were made, that he drew up a plan of his intended work, and laid it before the Royal Society; that body resolved to recommend the publication of it to the patronage of Prince George, who appointed a committee to report on the subject, of which Newton was the head, as president of the Royal Society. This committee, (who were called the Prince's referees,) were, (at least in Flamsteed's opinion,) wholly under the influence of Newton, or rather Halley. Their proceedings were viewed with the utmost jealousy by Flamsteed: yet it seems, that (as we have before seen,) complaining and grudging, he still allowed them possession of the MSS. of his observations; and in particular the catalogue of the stars, as yet in a very imperfect condition, was with much reluctance on his part, given, *sealed up*, into Newton's keeping, as a sort of pledge (we must suppose) for its completion, and with the express condition, that it was not to be opened till Flamsteed could supply what was wanting to its perfection. He was all the while accusing Newton, of a design to "gain the honour of all my pains to himself; to make me come under to him." . . . . "For *honest* Sir Isaac," (to use his own words,) would have "all things in his own power, to spoil or sink them, that he might force me to second his designs and applaud him, which no honest man would or could; and, God be thanked, I lay under no necessity of so doing."

When the printing was resolved upon, much delay occurred in carrying it on. This Flamsteed ascribed wholly to Newton.

It will not be at all within our limits to notice all the various sources of complaint and vexation, to which either party were not backward in referring. Some articles of agreement were drawn up which Flamsteed signed; but was always complaining that they were not adhered to by the other party. In December, 1707, the first volume, (containing the Sextant observations) was completed; under the title of *Historia Cœlestis*. The second was to be the more important, as comprising the observations with the mural arc. The terms of the agreement in this case, were such as Flamsteed had good reason to complain of; the sum awarded him, being, in fact, less than he had spent in the necessary means of carrying on the calculations. He considered, however, "If I should not consent to this order, Sir I. Newton would say, that I had hindered the printing of my own works myself, which would serve to justify a report spread by his partisans very industriously, that I was averse to the publication of them; . . . . further, I saw that if I did not lay hold of this opportunity, I could not hope to be reimbursed any part of the money I had spent," &c.

We will give this part of the narrative in Mr. Baily's words:—

"After a great deal of unnecessary procrastination on the part of Sir Isaac

Newton, a meeting with the Referees was appointed to take place on March 20, 1707-8, when Flamsteed took up with him the whole of the observations made with the mural arc, from September, 1689, to December, 1705, fairly copied out on one hundred and seventy-five sheets of large paper, *together with a more extensive and perfect catalogue of the fixed stars.* At this meeting new articles were suggested, and finally imposed upon Flamsteed: for he was not only obliged to leave the whole of the one hundred and seventy-five sheets of manuscript in Newton's hands, *but also bound himself to complete, and return within sixteen days, the catalogue which had previously been delivered, sealed up to him: Sir Isaac returning the one which Flamsteed had brought with him, as a pledge for the performance of the contract.* Notwithstanding this compliance, however, on the part of Flamsteed, the work of the press does not seem to have been expedited: further obstructions were thrown in the way of proceeding, the nature and cause of which are not sufficiently apparent; and Prince George died, (October 28, 1708,) before the second volume was entered upon. The work was now completely stopped; and although by this melancholy event the power of the Referees ceased, the papers were still left in their hands."—Preface xxxvii.

Flamsteed, meanwhile, industriously continued his observations, until, in 1710, "he was afresh disturbed by another piece of Sir I. Newton's ingenuity." On Newton's recommendation, the Queen appointed the president, and some members of the Royal Society, *visiters* of the Observatory at Greenwich; investing them with considerable powers over the Astronomer Royal. This called forth all the vehemence of Flamsteed's indignation. He maintains that the visiters were incompetent, as knowing nothing of practical astronomy; and complains that he was worse used than Tycho, (to whose ill treatment he often refers;) he remonstrated with the secretary of state, but was "answered haughtily that the Queen would be obeyed." He petitioned the Queen herself: alleging, that the visiters knew little of his business, and would but incommode him in his progress, and obstruct him: he finally prays, that such of the nobility or gentry as are skilful in mathematics, together with the officers of the ordnance, may have the care and inspection of the Observatory.—(p. 279.)

But besides this, the other grievances of the affair of printing were soon renewed. The Queen undertook to carry on the design of the late Prince, and the same persons continued, in fact, to manage the business, though Dr. Arbuthnot was nominally commissioned for the purpose They accordingly proceeded with the materials of the second volume, without consulting Flamsteed. On learning their proceedings, his indignation was roused to the highest pitch: he called the offer of the Queen's patronage, "throwing chaff before him." He remonstrated in the strongest terms, and again petitioned the Queen; though without effect. He soon found that the board had proceeded to print the catalogue of the stars, which he had left in their hands as above stated, and not only this, but the observations also mentioned as contained in the one hundred and seventy-five sheets, were put to the press in an incorrect and garbled form; certain observations only being *selected.* And further, the places of the moon which were inserted, turned out to be, in part, the same which he had at the very first given to Newton, under a promise of secrecy, as being derived only from imperfect determinations of the places of the stars.

It seems that the proof-sheets were communicated to Flamsteed, and amid abundance of complaints, he further says, "I was willing to have filled up the copy of the catalogue, but perceiving hereby that Halley was minding to spoil the work, and with more views than one or two, I sent Dr. Arbuthnot an account of his villanous outrage, and desired he would permit me to print my own catalogue at my own charge."

Flamsteed, however, had not sufficient interest to stop the press; for the work thus, as he considered it, *mutilated and corrupted*, ultimately appeared in one volume, accompanied with a sort of exculpatory preface by Halley, who superintended the edition.

All the grievances and delays which occurred, were caused, in Flamsteed's apprehension, through Sir I. Newton's only procurement; for "to keep all things wholly in his own power, he had brought in an undertaker, who was useless to the business, and a printer, whom I believe he paid." This last remark, at least, shows Newton's good-will in the cause.

But that which Flamsteed felt as most nearly touching his astronomical reputation, was the garbling of his observations: he describes them in a letter to his friend Sharp, as—

> . . . ." corrupted and spoiled by Dr. Halley. He is as lazy and slothful as he is corrupt. With my lunar observations he gives her true places and latitudes, which are copied from the three large synopses that I imparted to Sir I. Newton, under this condition, that he should not impart them to anybody without my leave. Yet so true to his word, and so candid, is the knight, that he immediately imparted it to Halley; who has printed them as far as they reach; and afterwards thrust in the moon's places from the ephemerides, or rather, I believe, from the margin of my book of observations, which is now in his hands; for the lazy and malicious thief would scarce be at the pains to gather them himself from the almanacs."—(p. 322.)

Flamsteed thus justly incensed, it may be well supposed, was in no very favourable state of mind for meeting the visiters of the Observatory, who comprised in their number the leading individuals of the referees. At a meeting which he was summoned to attend, (1711,) a scene ensued which evidently dwelt long on his mind, as he has described it three or four times in his diaries and letters; and which, certainly, appears at the present day, one of the most singular which can well be imagined. Newton (as president,) put some questions about the state of the instruments, on which Flamsteed with some warmth reminded them, that the instruments were all his own, and then ran on to the publication of his observations, whereby, he said, "he was robbed of the fruit of his labours."

> "At this, (he continues,) the impetuous man (Newton) grew outrageous, and said, 'we are then robbers of your labour?' I answered, I was sorry they owned themselves to be so. After which, all he said was in a rage. He called me many hard names; puppy was the most innocent of them. I told him only, that I had all imaginable deference and respect for her Majesty's order, for the honour of the nation, &c; but that it was a dishonour to the

nation, her Majesty, and that Society, (nay, to the President himself,) to use me so. At last he charged me with great violence, (and repeated it,) not to remove any instruments out of the Observatory: for I had told him before, that if I was turned out of the Observatory, I would carry away the sextant with me. I only desired him to keep his temper, and restrain his passion, and thanked him as often as he gave me ill names. . . . . I cannot remember everything that was said by this hot gentleman in its proper place, nor have I given it in its order. I may put it into better upon recollection hereafter."—(p. 228.)

This he did in a subsequent letter, December 22, 1711, (p. 293,) in which he gives nearly the same narrative, but with this remarkable addition.

"I only told him, my catalogue, half finished, was delivered into his hands on his own request sealed up. He could not deny it, but said, Dr. Arbuthnot had procured the Queen's order for opening it. This I am persuaded was false, or it was got after it had been opened."

He repeats this last circumstance in "a draft of a petition" to the Queen, (p. 295,) thus—

"Mr. Flamsteed being unwilling to part with it, (the imperfect catalogue,) one of them proposed to have it put into his hands sealed up, to which Mr. Flamsteed consented, and a part of the catalogue was accordingly put into his hands, March, 1705-6, after which the press set to work. . . . . . . Some time after, Mr. Flamsteed was told that the copy of the catalogue was opened and unsealed, which he could scarce believe."

This statement is adopted by Mr. Baily in his prefatory narrative, and commented upon with some severity, as a most unjustifiable breach of faith.—(Preface, p. xliii.)

After this occurrence Flamsteed, indignant at the treatment he had received, broke off all communication with the referees. He drew up a statement of all the proceedings, with a view to publishing it in his own defence; and proceeded, with the most resolute spirit, to revise all the observations, to supply deficiencies, and to correct the errors of the printed catalogue, with the determination of publishing a perfect copy at his own expense. He got it through the press in 1712: and then resolved on continuing the remainder of his observations, which had not yet been published by the board. He demanded, of Newton, the restoration of the papers in his hands: this was refused: he commenced legal proceedings for their recovery: the result does not distinctly appear; but it is certain that some of them were eventually returned to Flamsteed. One portion had been handed over by Newton to Halley, which Flamsteed calls "the height of trick, ingratitude, and baseness." He had however the original entries, and proceeded, at his own expense, to have them recopied and arranged for the press.

The state of affairs changed materially with the death of Queen Anne, in 1714, followed, as that event was, by the death of Lord Halifax, Newton's great patron at court. Flamsteed had now more interest, which he exerted successfully, in procuring an order to have delivered to him all the copies of the mutilated work remaining (300 in number), these, he tells us, he carried home in triumph, and made "a sacrifice of them to heavenly truth," by committing them to the flames, "that none might remain to show the ingratitude of his countrymen, who had used him worse than even the noble Tycho was used in Denmark."

It was, however, only *a part* of each copy which he destroyed, viz.: the garbled catalogue and mural arc observations: the first part, or sextant observations, remained. This, with some additional observations, which he proceeded to print, now forms the first volume of the *Historia Cœlestis.* The remainder filled two more volumes. Flamsteed had advanced nearly through the second volume, when his increasing infirmities and death put a stop to the work.

Flamsteed died in 1719. Halley was appointed his successor; but even allowing for considerable prejudice on the mind of Flamsteed's friend, Crossthwait, it would seem, from one of his letters, that Halley was in most indecent haste to remove Flamsteed's widow from the observatory, and to take possession; so much so, that he speaks of having "met with much trouble from him," and, in the hurry, being obliged to remove Flamsteed's remaining papers in great confusion.

Crossthwait and Sharp, however, jointly superintended the printing of the remainder of the work, without any remuneration whatever to themselves, on behalf of the widow, who, with the most praiseworthy regard for her husband's honour and fame, determined to finish it at her own expense. The whole was completed in 1725, except the maps of the constellations, which were not finished till 1730. And lastly, it should be added, that a copy of the spurious edition having been placed in the Bodleian library, at Oxford, Mrs. Flamsteed addressed a spirited letter to the vice-chancellor, begging that it might be ejected.

Such is the outline of this singular history, which we have endeavoured to give in as impartial a manner as possible. We think but one impression can remain as to the temper evinced by Flamsteed throughout, and that he viewed all the circumstances through a distorting medium. At the same time, there was enough of injustice in the proceedings of the other parties, to have stirred up a quieter spirit than his.

The *Edinburgh Review* has stated fairly the characters both of Newton and Flamsteed; though not (as appears to us) fully enough, to give much elucidation of the strange positions in which they appear to be placed in Flamsteed's narrative. As to Newton, it is clear that we have long known him through the representations of the most partial witnesses only. The most popular accounts have been derived from the *Eloge* of Fontenelle, the materials of which were supplied by Newton's nephew and heir, Conduit. Whilst English writers, in general, have been so completely dazzled by the splendour of his philosophic greatness, that they have been as unable as unwilling to perceive the shades which may partially obscure his character. The extreme equanimity and mildness of disposition which has been commonly ascribed to him, it is well observed by Mr. Whewell, "showed itself rather in his horror of disputes than in his skill in conducting them." And with equal truth he speaks of "the indignant and garrulous statements given by poor Flamsteed, under the irritation of a fretful temper, hard work, old age, ill health, and constant quarrelling."

The Edinburgh reviewer observes, "Flamsteed was one of those *unhappy* individuals, who, from an unfortunate idiosyncrasy, are never, for any length of time, free from some cause or other of annoyance and molestation." And Mr. Whewell characterizes him as "a good and conscientious man, but of weak health from childhood; he seems, also, to have been of weak temper, suspicious, irritable, self-tormenting." And, in the remarkable scene, at the board of visiters, "according to his own account, he began by calling them robbers of his property." In describing the altercation which ensued, he says, "I *only* desired him (Newton) to keep his temper, restrain his passion, &c.;" and such expressions are more than once repeated. "We hardly require the recollection of Sir Anthony Absolute to see here the demeanour of a very angry man: far too angry, certainly, to allow us to accept literally what he asserts, much less what he implies merely. I confess that I have great doubts whether, from the expression in the same account, 'he called me *puppy*,' &c., we can confidently infer that the obnoxious term was used."—(Whewell, p. 12.)

We think that the tenour of many extracts we have given in the course of the narrative, all confirm this view of the case. Flamsteed, however, is described as a man of singular piety: and, indeed, his diaries and letters abound with devout expressions of thankfulness to God, and forgiveness to his enemies; that these contrast strangely with his complaining temper, and bitter and rankling enmity, is, unhappily, but too consistent with human nature.

Mr. Whewell has put strongly the case of Newton, in the very critical moment of developing the lunar theory, and in that state of intense anxiety (which, perhaps, none can properly appreciate but those who have been engaged in the verification of a philosophical theory), to obtain the necessary data from observations, which Flamsteed alone could supply; and which, imperfect and approximate as he might think them, (and perhaps justly, considering the fastidious exactness at which he aimed,) were of essential use, in that stage of the investigation, for establishing the first general agreement of the theory with the phenomena. Of the nature of Newton's researches, Flamsteed (as we have said) entertained very imperfect conceptions; and there are abundant evidences of this in all parts of his letters, which are well brought together in the pamphlet before us.

As to the increasing coldness, and at length open rupture, between the parties, it certainly appears that Newton endeavoured, as long as he could to prevent it: while Flamsteed's increasing tone of bitterness is not more apparent than the clear disclosure of that unhappy inherent disposition which delighted in brooding over grievances, real or supposed. He was in the habit of appending marginal notes to the letters of Newton: thus (*e. g.*) on a casual expression, "I believe you have a wrong notion of my method in determining the moon's motions," &c.; he puts in the margin, "I had: and he of me: and still has."

Halley, early in life, had acquired reputation as an astronomer; and by his numerous and varied scientific pursuits and theories, had also obtained a considerable share of popular fame, of which Flamsteed appears to have been not a little envious. He was certainly the best practical astronomer, next to Flamsteed, of the day, or in Flamsteed's words, "knew *more of his business*" than any man.

"Hence (as the Edinburgh reviewer observes,) his anxiety, of which some amusing instances occur, to keep him in ignorance of what he was doing. In addition to all this, Halley was of a jovial and convivial disposition, fond of society and enjoyment, and from Flamsteed's remarks on his disposition to raillery and banter, we may suppose that he sometimes exercised those talents at the expense of the astronomer; for which, indeed, the numerous salient points of his character afforded abundant temptation. Yet these companionable qualities would seem to have produced their usual effect, even on the reserved and precise character of Flamsteed.

"'I hate his ill manners, not the man: were he either honest, or but *civil*, there is none in whose company I would rather desire to be.' It has been surmised, that Flamsteed's aversion to Halley arose from the libertine conduct and infidel opinions which the latter entertained, and took no pains to conceal. We have no evidence of this. At all events, if Halley's acquaintance was so disreputable, what is to be said for Newton and others, with whom Halley remained so many years in terms of intimate friendship*?"

As to Halley's infidelity, it is possible the reviewer may be quite correct in saying there is no evidence of it; that is, of his professing, deliberately, the doctrines of speculative unbelief. Yet in another view his irreligion may have been sufficient to justify the accusation of Flamsteed, as showing itself in licentious and profligate habits. That his conversation was frequently characterized by profaneness and impiety, appears from the circumstance, recorded on good authority (Brewster's *Life of Newton*, p. 339), that he was often rebuked by Newton on this score. We can easily conceive then, on this ground alone, how obnoxious he must have been to a man of Flamsteed's conscientious piety, who scarcely entered a memorandum in his diary without a devout expression appended to it. But the cause of his dislike was more deeply seated. He clearly saw in Halley a *rival;* the only man of the day who came near him in his own science, and who, from the first, was throwing out insinuations and accusations against his proceedings at the Royal Observatory; and alleging that, as the observations were not published, it was useless to the nation. He was the mover in all the measures for *inquiry* and superintendence, which Flamsteed, conscious of his own rectitude and gratuitous exertions, so highly resented; but which those, who did not understand the nature of his labours, and indulged in *suspecting abuses*, no doubt looked upon as highly proper and necessary. Halley all along had an eye to the place of Astronomer Royal himself, at any rate on the death of Flamsteed, and possibly by worrying him out of it during his life: (Thus, "June 18, Dr. Halley came . . . . . asked if I wanted preferment (a snare!)" . . .) The manifest suspicion of all this in the mind of Flamsteed, is enough to account for any degree of dislike: whilst it supplies a clue to the proceedings of Halley.

We have said Halley appears to have been the prime mover in all the obnoxious measures. We know that he was a busy, energetic man, always urging on (often to very good purpose, as in the publication of the *Principia*,) the designs of others: but from his want of principle, we may infer that he was not likely to be over scrupulous in the means he employed. Hence we need not be surprised if deviations from strict honour or rectitude had taken place in transactions, of which he was the mover, especially when an important end, connected with the advance of science, was to result.

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* *Edinburgh Review*, No. CXXVI., p. 394.

But he did not appear as the ostensible mover in the proceedings to which we have referred: the whole blame was made to fall on the devoted head of Newton. If we look behind the scenes, however, we shall find at least suspicion enough, that Newton was almost wholly under Halley's influence. In the first place, his just obligations to him were of no ordinary magnitude, Halley having taken upon himself the entire trouble, and, as would appear, also the expense, of printing the *Principia**. Halley was a man of great practical skill in the management of affairs, he knew human nature; and in social intercourse possessed an influence, which (it appears from a previous extract) even Flamsteed was constrained to own. It is, therefore, easy to account for his ascendancy over Newton, when he had many purposes to serve, both for the interests of science and his own ambition, in connecting himself with an individual so eminent, and who possessed so powerful an interest at court.

Newton's was precisely the character to afford every facility to his designs. Quiet and retiring, reserved and timid from a morbid sensitiveness of mind, he was easily influenced by bolder spirits. Abstracted from society, and absorbed in study during his younger years, he was extremely ignorant of the world and of mankind; and thus desirous to shun its dissensions, yet easily drawn into them by a designing instigator; and when engaged in them, utterly unable to steer a steady course; and feeling annoyed and uneasy at his own position would be very likely to give expression to his vexation in acrimonious language, without discriminating occasions or persons. We also readily perceive in the constitutional morbid reserve, jealousy, and suspicion, which always attached to his disposition, an ample explanation of the slight but highly characteristic sketch given us by the master-hand of Locke, "he is a nice man to deal with†."

These are a few considerations which we have ventured to throw out, grounded merely on the general view of the manifest characters of the distinguished persons, who act parts in the scene presented to us in Flamteed's *Memoirs*. Newton was intensely anxious for the publication of the observations: Halley was equally so—had other designs in view at the same time, exercised an ascendency over Newton, took advantage of his simplicity, and urged him on to practices which he no doubt managed to persuade him were all justifiable, especially under the plea of the royal authority of his commission. Flamsteed had much just cause of complaint, but grossly exaggerated it. The final outbreak of virulence and mutual abuse, partook of the coarseness of the age.

The publication of Flamsteed's observations was after all not *wholly* against his consent. We have more than once observed him bitterly remonstrating, but nevertheless giving up his papers. Prior to the appointment of the visiters, it certainly does not appear why, if really so fearful of having them printed, he should not steadily have refused them. But with respect to the want of instruments and insufficient means, why, on finding an appeal to the throne useless, did he not resign? He might then have retired to his living, and without greater pecuniary loss than he actually incurred, have prosecuted and published his observations in dignified independence. But this would have placed Halley at Greenwich: Flamsteed would have endured any extremity rather than this.

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* Birch's *History of the Royal Society*, vol. iv., p. 486.

† *Lord King's Life of Locke*, vol. ii., p. 38.

When the appointment of visiters took place, one special point in their commission was a power to *demand copies of the observations.* With this he was backward in complying; not wishing one portion of his observations to go out of his hands until perfected and corrected by others. But at this rate he might have delayed indefinitely: since no set of astronomical observations is perfect; and, in all cases, even the best remain to be corrected by subsequent determinations. The queen's authority was precise, and the public service required data for the uses of navigation.

The main accusation against Newton is twofold: first, a breach of confidence in printing the copies of lunar observations, long before given him as a private friend, in his public and joint capacity of visiter and referee; those papers having been also given under express stipulations of secresy. Second, a more glaring act of dishonesty in breaking open a sealed packet, which had been deposited in his hands officially, and printing the contents. The first charge we conceive will admit of no direct justification, however it may be extenuated by the delay of Flamsteed in furnishing copies of his observations, and the authority which the visiters lawfully possessed to demand them. With respect to the second, it has been regarded as decidedly the most flagrant and unjustifiable portion of the charge. It has been described thus by Mr. Baily; it has been made the foundation of the heaviest accusations by the Quarterly and Edinburgh Reviewers, and is not at all denied or cleared up by Mr. Whewell, though he seeks to justify it by the authority of the royal commission.

We were particular in giving precisely the statements extracted from Mr. Baily's volume, which relate to this incident; and it appears to us, to say the least, that there is a *strange inconsistency* in the evidence. In the memorials at the time of the transaction, Flamsteed makes no mention whatever of this breaking open a sealed deposit. It is not till some time after, in a moment of the most violent passion, that he utters such a charge; and even this he recollects only in a *second* and revised version of his story. This might leave it open to a considerable degree of suspicion. But the strong circumstance is, that in the former narrative of the agreement with the referees, (which we gave in Mr. Baily's words,) it expressly appears that the sealed catalogue, which had been before deposited in Newton's hands, *was returned to Flamsteed* UNOPENED, and another catalogue more complete was delivered to Newton without the mention of any restrictions; Flamsteed engaging to fill up the former and return it. It would thus appear that this, confessedly the worst, part of the charge against Newton, from such an entire discordance in the evidence adduced, must fall to the ground.

THE BRITISH ASSOCIATION, AND THE QUARTERLY REVIEW.

We alluded in our opening article to the low esteem in which science is often held among those who *par excellence* arrogate to themselves the title of the highly-educated classes. We often perceive proofs of it in the spirit occasionally manifested by those periodicals which are under the especial patronage of the more polite portion of society; not to mention the obscurity or total neglect to which real science is generally consigned by the daily press; varied only by occasional puffs of all that species of philosophy, which, from its intrinsic emptiness and levity, least requires such aid to raise it in the gross atmosphere of popular fame.

But the circumstance which has led us to these topics at the present moment, and which is intimated by the heading of this article, is the occurrence, in the recent number (cx.) of the *Quarterly Review*, of some matter which appears to call for our more special animadversion.

This learned and refined organ of literary exclusiveness, does now and then condescend to acknowledge that there is such a thing as science; and that it may not be utterly degraded by allowing some obscure corner of its pages to discussions connected with these subjects. Consequently in the present number, the subject of M. Agassiz and his discoveries relative to fossil fishes is selected, and very justly, as one highly interesting in itself, and susceptible of popular illustration. Of the manner in which the subject is treated it is far from our intention to say anything. We shall only observe, that towards the end of the article the writer takes occasion, very properly, to refer to the necessity for pecuniary assistance for carrying on a work so vast, and at the same time so little likely to be profitable, as that which M. Agassiz is engaged in bringing out, in successive numbers, containing beautiful plates of the different species of fossil fishes. Indeed, (by the way,) we more than surmise the whole of the short article referred to is mainly designed to lead simply to this one point, of soliciting the public to subscribe. Nor do we at all blame such a procedure: it is highly praiseworthy; and we shall be heartily glad to concur in promoting so desirable an object. We merely remark this as tending to confirm our impression that had there *not* been such an object in view as the support of an individual, whom the polite journal condescends to patronize, (though engaged in the extremely unworthy occupation of groping and grovelling in the earth to disinter fossil remains,) we should probably have heard little of his important discoveries;—though perhaps not less than we actually do, of their high philosophic bearings.

To return, however, to our immediate subject: This recommendation of subscription is introduced by a mention of the circumstance that a sum was awarded for this purpose by the British Association for the Advancemence of Science, at the Dublin meeting: which is prefaced by the following remark:—(p. 444.)

> "We are not among those who are in raptures with the British Association for the Advancement of Science. It delights in greater display than becomes the modesty of philosophers: nor do we think that their mutual bepraisings—their Amœbean eulogies—are at all likely to add to their dignity. Wherever

they go,— Earth no such sons, no folks have such a town:' and we cannot view with feelings of complacency our scientific Samsons, led forth to make sport at its festivals; even though the exhibition should be hallowed by a few sprinklings from the fountain of honour, distributed through the *spout* of Ireland's Viceroy."

To show his laudable candour, the writer continues—

" While, however, we do not conceal our opinion of its faults, we must not be blind to its merits," &c.

And then comes the mention of the vote of money to M. Agassiz.

We will take these charges in order:—1st. The Association is accused of greater display than becomes the modesty of philosophers. Now what sort of *display* does the reviewer mean? Is it the display of the results of researches, which the members have been carrying on during the year, many, if not most of which, have been undertaken at the suggestion of the Association, and of which a public statement must be made in return? We hardly suppose this can be what is meant: yet most certainly such *display* as this is what, in fact, constitutes the great mass of the proceedings: a display of the results of philosophic labours, made to the assembled body of cultivators of science; and that not for any purpose of display as such, but to call forth discussion, observation, very often criticism and censure. The *display* of *science* before the *public*, perhaps is meant; and that philosophers, in virtue of that "modesty" which ought to be so peculiarly theirs, should keep all their discoveries to themselves, or, at least discuss them only in select coteries, but never proclaim them to the world. But if they hold such meetings among themselves, and the public desire to be admitted, would the reviewer wish to exclude them? If a mixed audience, including numbers of the fair sex, choose to be present, who would desire to prevent them? Nay, even granting as much *display* in the thing as may be wished, what is the object? Is it not the promotion of something like a just recognition of the claims of science, in which public opinion has generally been so notoriously deficient? and if for this it be unavoidable to run into the slight degree of display which attends a general gathering of men of science, and the public exposition of scientific truths at their assemblies, it is difficult to see how any one can blame it, unless it is on the ground that Science does not *need* such a stimulus; an idea which can only be entertained by those who are profoundly ignorant of its actual condition. It is a *display*, too, not merely to the public, on the part of the philosophers, but anxiously desired by the junior and humbler cultivators of Science, from their elder and more distinguished fellow-labourers. The numerical majority attend these meetings with the view of *gaining* information, (we might say, with truth, all do so in some sense;) which they cannot do unless it be publicly and formally put before them. The British Association, we affirm, does not, as the reviewer asserts, "*delight* in display." Some publicity is necessary for its object, and if the attendant degree of display be an evil, it is a necessary evil.

But further, "the mutual bepraisings," the "Amœbean eulogies" of the philosophers, "do not add to their dignity." We should be glad to compare the proceedings of the British Association, in this particular point, with those of any other large public institution which holds annual meetings. We imagine the "bepraisings" which take place in any other great meeting are not only, at least, fully as numerous, but generally far more vehement, in proportion as they usually refer to topics in which the feelings of the parties have so much more to do. We do not well see how a great meeting is to be carried on without some attention to the customary decencies of courteousness: without some such things as votes of thanks to officers, to benefactors, to supporters of the cause; some notice shown towards persons of eminence who may be present; and the like. Nor will the customary expectations of a mixed audience allow such occasions to pass without the occurrence of many expressions of eulogy, which are well enough carried off by the excitement of the moment, however little they might bear the test of severe criticism on cool reflection afterwards. But there is one thing peculiar to the "Amœbean eulogies," in the particular case in question, which is often overlooked by those (and there are many such) who make objections on this ground; viz., that in some (and those the most marked) cases, this eulogy is not mere unmeaning flattery, but is rather the pronouncing of the judicial sentence, as it were, of a great scientific tribunal on the pretensions of some aspirant after philosophic honours, whose researches have previously formed the subject of close examination and debate: that, then, which appears, to an inconsiderate bystander, the mere effusion of flattery, is often nothing less than the united decision of the assembled Science of the country, on the labours of some retired cultivator of Science, which thus receive the stamp of public opinion from those best qualified to judge of their merits:—the highest recompense of scientific toil, and the surest stimulus to renewed and well-directed exertion.

As to the compliments paid to those whose liberal hospitality welcomes the Association wherever it goes, and even contends (often with no slight warmth, as we could witness,) for the priority of selection, we can only ask how is it possible for any individual or any body, received as the Association and all its members have been, to abstain from uttering those thanks, which they must be unfeeling indeed, not to perceive are barely an act of justice? We will merely add, on this topic, that though the reviewer is evidently glancing at the Dublin meeting, he clearly has no right to talk of subjects of which he is ignorant: the very fact of his penning the words alluded to, amounts to a positive proof that *he* was not among the visiters on that occasion, or he *could* not have written them.

"The Samsons led forth to make sport at its festivals," is, doubtless, a most humorous and right merry conceit; but we should be glad if the writer had produced a few instances of the sort of *sport* he alludes to. On casting a rapid glance over the report of the proceedings of the last meeting, we tried to recognise some of these Samsons who figured in the Rotunda, and to see what sort of sport they made. The grave and learned opening speech of the venerable President was, we should imagine, anything but *sport*. The brilliant eloquence of Sir W. R. Hamilton's address (delivered officially as secretary), we may allow so far, was indeed the production of a Samson. We do not know what exhibition of this kind could be found in the masterly exposé of Mr. Whewell on *the Tides; or in the* lucid, but by no means sportive, lecture of Dr. Lardner on Rail-roads. We could discover no particular comic display in the philosophic observations of Professor Babbage; nor in the grave discussion on Light of Professor Powell. If there was anything approaching to sport, it was in the address of Professor Sedgwick; but it was not the *sport* afforded by a blinded and fettered Samson. For our own parts we think, if, under this ridiculous phrase, it be meant to complain that the public lectures and expositions of Science were of *too popular* a kind, it merely shows how little the Quarterly reviewer appreciates the value of a popular diffusion of a taste for physical knowledge, and the extreme importance of enlisting in this service the talents of really sound and eminent scientific men, instead of mere shallow declaimers and ignorant empirics. One of the grandest objects of such an Association, we are persuaded, is that of promoting, not only the abstract researches of the *few*, but the dissemination of a knowledge of them among the *many:* to secure to Science something like the attention it ought fairly to receive, and the estimation it ought to command, in public opinion. And so far from thinking there is *too much* of popular display at its meetings, we are persuaded, on the contrary, there has been (at the two last meetings especially) *too little* opportunity given for the public exposition and enforcement of these great objects.

The miserable pun in which the Representative of the Sovereign is here insulted by the *loyal* Quarterly, shows clearly the feeling which dictated the attack: the writer is anxious to fix a *political* stigma on the Association, and this is his proof!

ON THE ELEMENTARY STUDY OF GEOMETRY AND ALGEBRA.

There are few questions in reference to which writers on elementary mathematics have been more divided, than that between the Geometrical and Algebraical methods. But though able disputants have contended strenuously on each side for the superiority of the respective systems, they have contributed very little to a clear exposition of the essential points of difference.

The question refers not merely to the comparative advantages afforded by the two systems, but even to the absolute validity of the processes of reasoning which they employ. The external characteristics of style, method, and notation, doubtless present many points of discussion and comparison: even these have been by no means well contrasted, and a very discordant variety of representations may be collected from the writings of the most eminent men who have drawn such comparisons. But these are distinct from the more important point in dispute, which relates rather to the fundamental principles assumed, and the nature of the reasonings adopted in each system, and here it is that the discussion has been most defective. From the writings of the most distinguished mathematicians, it is very difficult to derive any satisfactory or precise notion of these more essential grounds of distinction: and it may with truth be said, that though many have contested whether

Algebra or Geometry be the best, but few have discussed, and none have clearly pointed out *what is* Geometry, and what Algebra.

The subject, indeed, has been but incidentally introduced by most of these writers, and a complete investigation of it has been long a desideratum in mathematical literature. By some it has been alleged as the distinction between Geometry and Algebra, that the demonstrations of the one are synthetical, while the processes of the other are analytical. But in any sense of the words, this is far from being universally or even extensively applicable: and did it obtain, it could in no way affect the *validity* of either method. But in truth, so wide has been the latitude of meaning in which the terms analysis and synthesis have been applied in these departments of Science, that it is difficult to fix upon any determinate particular in which the distinction can be understood to consist.

By others it has been made the point of comparison, that in the one system we reason independently on each particular case, by a method limited to that case, whilst in the other, we avail ourselves of general methods, applicable at once to all cases; and when the first data are established, affording the facility of an almost mechanical process for arriving at results, to which Geometry, if at all, could only conduct us by a slow and laborious advance. Thus aided, the analyst is enabled with ease to overcome difficulties, with which the Geometer might often struggle, single-handed, in vain. The methods of Geometry have been compared to manual labour, those of Algebra, to manufacture by machinery: the one to a journey performed on foot, the other to a steam-carriage on a railroad.

In all this there may be much truth of description, but nothing by which any essential distinctive principle is brought to light.

Some authors characterize Geometry as not requiring that abstraction which analysis demands, and directly addressing the senses as well as the understanding. Others represent analysis as superseding thought by mechanical processes, and thus rendering investigation easy to those who are incapable of the abstraction which Geometry requires.

Some censure an exclusive pursuit of Algebra, as tending to narrow the mind, by confining the attention to the more individual steps as they succeed one another, and to the routine of set rules, without requiring or allowing any extended and comprehensive views; while Geometry, it is alleged, expands and invigorates all the mental powers, by the healthful exercise of unfettered invention.

Others, on the contrary, select this as the very ground of commendation, that Algebra expands and generalizes our conceptions, while Geometry contracts and restricts them to the consideration of isolated cases.

The two methods are characterized by Professor Playfair, from "the different modes they employ to express our ideas. In Geometry, every magnitude is represented by one of the same kind; lines are represented by a line, and angles by an angle; the genus is always signified by the individual, and a general idea by one of the particulars which fall under it. . . . . . In Algebra, every magnitude is denoted by an artificial symbol, to which it has no resemblance," &c. And, again, the same author remarks in another place, that all mathematical investigation must involve two things,—the quantities concerned, and the operations performed upon them. Geometry expresses the first by real representations, and the second by words at length. Algebra does both by conventional symbols; and the symbols of operation constitute one very important point of superiority in Algebra.

Of the vast facilities afforded by a symbolical notation, there can exist no question. But as far as the reasoning is concerned, algebraical solutions might be carried on in words at length, and were, in fact, actually so delivered by the earliest writers. The symbolical notation was the gradual result of the necessity for such abbreviations, which was felt in proportion as the operations became more extensive and complex.

Geometrical demonstrations, on the other hand, especially where there are complex figures to be considered, made up of many parts, which are to be added or subtracted, may with material advantage in point of perspicuity, be conducted, by adopting the use of the signs of addition, subtraction, and equality, as employed in Algebra, while the reasoning remains identically the same.

Other writers have put the distinction thus: that in Algebra the reasoning is general, since the symbols are so. In Geometry we select a particular case, on which to prove a general theorem.

But in point of fact, the particular figure, " the triangle A, B, C," for example, which we take to reason upon, is *not* a particular case, but merely *a name* given to the general idea of all triangles constituted under the same conditions, to assist the memory and the conception; the demonstration would be equally valid, only less intelligible, if conducted throughout in general terms, without reference to any diagram, or to any imagined particular figure.

Thus, as to any such distinctions, it is, in fact, evident, that as far as the actual reasoning is concerned, it may be carried on in Geometry without diagrams, and in Algebra without symbols; or in Geometry by symbolical notation, and in Algebra with diagrams (where the subject admitted them), and yet, all the while, the distinction between geometrical and algebraical reasoning might be perfectly preserved.

But the benefits of the algebraic symbols, have by others been considered as counterbalanced by attendant evils; and the absence of them in Geometry, as, in fact, securing it against the difficulties and paradoxes, which, it is alleged, have been introduced into Algebra, from the very comprehensiveness of its symbolical notation.

This, again, is answered by others who will not admit that Geometry *has* been thus preserved from defects in its chain of reasoning. For if the champions of Geometry have charged upon Algebra the admission of paradoxes and absurdities, and have censured the negative sign and impossible roots, the Algebraists have retorted upon Geometry the mysticism of some of its definitions and axioms, the indirect and circuitous methods of its demonstrations, and the primary deficiency in the theory of parallel lines.

Speaking of reasonings as expressed in particular, or in general terms, Mr. Dugald Stewart observes, " the former process is analogous to the practice of Geometers, who, in their most general reasonings, direct the attention to a particular diagram; the latter to that of the Algebraists, who carry on their investigations by means of symbols*."

He adds, in the course of a note on this passage, "The straight lines which are employed in the fifth book of Euclid, to represent magnitudes in general, differ from the algebraic expression of these magnitudes, in the same respects in which picture-writing differs from arbitrary characters."

Again, after alluding to the paradoxes into which mathematicians have sometimes been led from want of due caution in the interpretation of algebraic symbols, he remarks, that in Geometry, the use of diagrams effectually prevents the introduction of any of these paradoxes.

Again, let us take the representation of La Place. "Cependant, les considérations géométriques ne doivent point être abandonnées; Elles sont de la plus grande utilité dans les arts. D'ailleurs, il est curieux de se figurer dans l'espace les divers résultats et l'analyse; et réciproquement, de lire toutes les affections des lignes et des surfaces, et toutes les variations du mouvement des corps, dans les équations qui les expriment. Ce rapprochement de la géométrie et de l'analyse répand un nouveau jour sur ces deux sciences; les opérations intellectuelles de celle-ci, *rendues sensible par les images* de la première sont plus faciles à saiser, plus intéressantes à suivre."—*Syst. du Monde*, p. 423.

Another writer, after observing that a demonstration may be strictly geometrical though expressed by algebraical symbols, says, "Yet when the mind loses the distinct perception of the particular geometrical magnitudes compared, the evidence is similar in its impression to that of algebraical reasoning, in which the previous demonstration of the rules employed is the ground of our assent to the truth of the conclusions, and not the immediate perception of the geometrical magnitudes and their relations."

But the circumstance of the attention being directed to the symbol rather than to the idea for which it stands, would in no way alter the validity of the demonstrations, nor render them less properly geometrical. The sole essential question is, whether with the introduction of algebraic *symbols*, any processes dependent on *assumptions* of first principles peculiar to Algebra are introduced; and then we are, unquestionably, no longer pursuing geometrical proofs in algebraic language, but employing actual Algebra. Still the question remains, *What are those principles peculiar to Algebra?* and are they such as are in any way open to objection, either in themselves or in their application to Geometry?

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* *Philosophy of Human Mind*, Vol. I., p. 172.

[To be continued.]

ON AN INCREDIBLE EXPERIMENT IN WHICH THE HUMAN BODY LOSES ITS WEIGHT.

RELATED BY SIR DAVID BREWSTER AND ANOTHER.

HIGHLY exciting as the "marvellous" may be to a large proportion of mankind, even in the most advanced state of civilization yet known, it ought never to be drawn from sources whence truth may be obtained by ordinary industry of research, nor furnished by men whose *dicta*, from their intellectual rank, may be received, without examination or suspicion, not only by the ignorant and unreflecting, but by many who are in the habit generally of requiring proof whenever it is possible to be obtained.

To suborn nature and abuse knowledge, for the vulgar purpose of exciting surprise among the ignorant, can now never acquire more than a very short-lived success, and must, eventually, be productive of great humiliation.

Besides, thêre is abundantly sufficient among the grand, and even among the minute, operations and productions of nature, to satisfy the most ravenous appetite for the "wonderful" and the "new," without fabricating, or circulating, when fabricated by others, statements at utter variance with all known facts, and ushering them into the world in a manner tending to disturb that confidence in the constant uniformity in the laws of nature, which centuries of investigation have combined to produce, and upon which the philosophic mind reposes with satisfaction and delight.

Without meaning to impute ignoble motives to so eminent a philosopher, and so acute an observer, as Sir David Brewster, it is, at least, surprising to see him expose himself to a charge of this nature, and that, too, in a work whose very intention seemed to be the clearing of the mind's eye, the strengthening of its vision, and the increase, to borrow an astronomical phrase, of its penetrating power, so that it might pierce more thoroughly the mistiness which superstition and knavish cunning, sometimes for base, and frequently for criminal, purposes, envelop some simple, but little known operation of nature, or some refined, but only partially exposed, process of art. Sir David had also an abettor, if not an accomplice in the late Sir Walter Scott, but in him the love of mystification, and the practice of ingenious deception, were so predominant, that we rather wonder he was content to play so second-rate a part in the case which we are about to refer to.

In the "*Letters on Natural Magic*, addressed to Sir Walter Scott, Bart., by Sir David Brewster, K.H., LL.D., F.R.S., V.P.R.S.E., &c.," p. 255, &c., is the following passage:—

> "One of the most remarkable and inexplicable experiments relative to the strength of the human frame, which you have yourself seen and admired, is that in which a heavy man is raised with the greatest facility, when he is lifted up the instant that his own lungs and those of the persons who raise him are inflated with air. This experiment was, I believe, first shown in England, a few years ago, by Major H., who saw it performed in a large party at Venice, under the direction of an officer of the American Navy. As Major H. performed it more than once in my presence, I shall describe, as nearly as possible, the method which he prescribed. The heaviest person in the party

lies down upon two chairs, his legs being supported by the one, and his back by the other. Four persons, one at each leg, and one at each shoulder, then try to raise him, and they find his dead weight to be very great, from the difficulty they experience in supporting him. When he is replaced in the chair, each of the four persons takes hold of the body as before, and the person to be lifted gives two signals by clapping his hands. At the first signal, he himself and the four lifters begin to draw a long and full breath, and, when the inhalation is completed, or the lungs filled, the second signal is given for raising the person from the chair. To his own surprise, and that of his bearers, he rises with the greatest facility, as if he were no heavier than a feather. On several occasions, I have observed that when one of the bearers performs his part ill, by making the inhalation out of time, the part of the body which he tries to raise is left as it were behind. As you have repeatedly seen this experiment, and have performed the part both of the load and of the bearer, you can testify how remarkable the effects appear to all parties, and how complete is the conviction, either that the load has been lightened, or the bearer strengthened, by the prescribed process.

"At Venice, the experiment was performed in a much more imposing manner. The heaviest man in the party was raised and sustained upon the points of the fore-fingers of six persons. Major H. declared that the experiment would not succeed if the person lifted were placed upon a board, and the strength of the individuals applied to the board. He conceived it necessary that the bearers should communicate directly with the body to be raised. I have not had an opportunity of making any experiments relative to these curious facts; but whether the general effect is an illusion, or the result of known or of new principles, the subject merits a careful investigation."

The circumstances under which this narration is given to the public are such, that if the feat, said to have been performed, was not so utterly incredible, they would be amply sufficient to procure for the "wonder," ready circulation and unhesitating acceptation even among scrupulous observers of truth. Here a Major H. is stated to have performed the experiment successfully in Sir David's presence; and Sir David Brewster, F.R.S., addressing Sir Walter Scott, F.R.S.E., speaks of the feat as one "*you have seen and admired*," describes the experiment as one "*you repeatedly have seen, and performed the part both of the load and the bearer*," and "*can testify how remarkable the effects appear to all parties*," &c.

As Sir Walter never in his life-time publicly noticed this appeal nor contradicted the statement, he united his testimony to that of Sir David, and as if even this united evidence could be strengthened, there comes "over sea" another circumstantial account of the same feat, and that from a quarter in which no confederacy could be suspected, unless indeed the American naval officer, who taught it to Major H., at Venice, had carried it across the Atlantic, and up the St. Lawrence. In an American periodical, *Silliman's Journal*, No. 57, published in April, 1835, there is the following communication to the Editor:—

Kingston, Upper Canada, October 31, 1834.

"Sir,—As a subscriber to your valuable journal, I take the liberty of asking of some of your scientific readers the rationale of the following experiment.

"An individual is to place himself on a stool or a table on his back, with his arms and legs *crossed, keeping* the whole body stiff; four or six others are then to place themselves at about equal distances, by the sides of the first,—say two at the shoulders, two about the middle of the body, and the others by the hips and thighs. Extending the fore-fingers of each hand so as to touch the body, somewhat underneath. At a given signal, the whole party are to take as full an inspiration as possible, and at another given signal, simultaneously to respire very slowly, gently pressing the body upwards at the same time, when it will be found to rise with a very slight effort, and to continue rising until the breath is exhausted, when it will suddenly fall down with great force. The operators must be prepared for this circumstance, and immediately pass their arms under the body to break its fall; it will also be well for one individual to hold a pillow under the head for the same purpose. The experiment appears to succeed best in a closed room, and if the inspirations and respirations are not uniform, it will fail. I first saw it tried about twenty years ago, but have never yet heard or seen any satisfactory explanation of it.

"I am not aware that it involves any principle adverse to the known laws of gravitation, but it certainly appears for a short time to act independently of them. If you deem it (this letter) worthy of a passing notice, I should be glad to see it; if otherwise, let it be deposited in the Archives of the College of Laputa. "I am, Sir, respectfully yours,

"JAMES NICKALLS, Jr."

In this account respiration is one of the conditions, and the experiment differs in some other respects from Sir David's, not materially, however; but so far as it does, the feat is rendered still more improbable. This gentleman also states that he "*saw it tried about twenty years*" before, and still, in 1834, "*he had never yet heard or seen any satisfactory explanation of it!*"

What shall be believed, then, of this extraordinary fact, so extensively promulgated in the Old Hemisphere, and echoed back from the New? We agree with the editor, in the journal above referred to, that "it is desirable that it should be decided either that the appearance is illusory, or that a reasonable cause should be assigned," and also with Sir David Brewster, who says, at the conclusion of the extract given, that "the subject merits a careful investigation."

We have the satisfaction of laying before our readers an investigation and decision, made with a most careful attention to all the circumstances described by Sir David Brewster. We have been permitted by an "Experimental Society," which holds its meetings in London, to have access to that part of their minute-book, in which the introduction and investigation of this very subject, and the final decision of the society, are recorded. In order that the weight due to this investigation and decision may be properly estimated, we shall state, that though none of the members possess names which are to be compared with the splendour of those of the knight or of the baronet in question, yet some of them have distinguished themselves in the scientific world, and they all have a reputation for veracity, sufficient ability, habits of observation and patient inquiry, quite sufficient to qualify them to form a competent jury to try the question.

Though the members of this society systematically avoid notoriety as a body, the names of the members who assisted in this experiment may, in this particular case, be known, if any person should think it worth *while to express* the wish for them.

We also, for the same purpose of proving the confidence that may be placed in an investigation by these gentlemen, shall state shortly their mode of proceeding. When a subject is decided by them to be worthy of experiment, a director of the investigation is appointed, at whose command all the means that the society, as a body, or each individual so disposed, can furnish; and to avoid distraction and confusion, and ensure effective co-operation, his instructions are implicitly followed. At the conclusion of an experiment, made under these circumstances, the whole of the members present discuss the proceeding, and suggest any omissions they may have observed; if these are important, the experiment is repeated, and so on, until every doubt of every individual is removed, and unanimity obtained *. This result cannot always be arrived at by one experiment, or in one meeting; it was not on the subject in question, but the process is repeated until it is accomplished.

It was under such a procedure that the fact described by Sir David Brewster was examined. Almost every member of the society was, at one time or other, "*the load or the bearer*," but particularly the heaviest and the lightest persons of the number were always lifted. As might be expected, the opinions were various in the first experiments. The differences however became less and less as the investigation went on and the proofs were multiplied; and at length they entirely vanished. The final unanimous verdict of the society being, that no such effect was produced as that described in the *Letters on Natural Magic*, that there was nothing whatever remarkable produced by the mode of lifting: and that the facility which was acquired in the lifting was no more than might be expected from the promptness which the bearers, by practice, acquired in acting uniformly together, upon a given signal. The feat of raising and supporting even their most minute member upon the fore-fingers of six persons, they found quite impracticable.

If this verdict should, by any accident, reach the ear of Sir David Brewster, and he should think any further trial necessary, we are authorized to say, that the "Experimental Society" wish it to be understood that they are ready to undertake it, under any modification that he may be kind enough to suggest; and, after following his instructions with the most scrupulous accuracy, to state the result to the public.

Until some such re-agitation of the question should take place, we think after the above investigation, it must be admitted that the appearances described by Sir David Brewster were illusory, and that no reasonable cause can be assigned which will produce such effects. We think, also, that scientific men should abstain from giving currency to such monstrous improbabilities, unaccompanied by refutation or explanatory remark. It would be far better to continue the inquiry into the cause, either of the fact or of the error, and abstain from publication until some satisfactory information had been obtained.

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* We beg to suggest the universal formation of such societies. In a wish of assisting in such an important object, we hope to be able to give the constitution, &c., of the one we refer to.

DESCRIPTION OF THE CURRENT-METER,

AS RECENTLY IMPROVED BY MR. SAXTON.

THE necessity of accurately ascertaining the velocity of rivers, &c., in the numerous cases where the rate of the current, the total volume of the passing water, &c., are required to be known, has made it extremely desirable to have a convenient means of measuring and comparing these velocities, that may be applied in every case likely to occur.

Instruments for this purpose have been designed and described by Eytelwein, Wattmann, Fontaine, and others; but it is believed that no one of them has so well satisfied the conditions which have been latterly supposed to be necessary in this kind of instrument, as that which is the subject of this communication. This meter has been in the hands of several eminent hydraulic engineers, both English and Foreign, for the last two years, and has been employed successfully by some of them in investigations of great importance; in that, for example, of estimating the sources from which it has been lately proposed to supply the metropolis with wholesome water, its use was extremely serviceable.

Among the conditions which it seems desirable that an instrument intended for such purposes should possess, the following appear important: Facility of use under all possible circumstances; portability; fewness of parts; strength and simplicity of construction, so as not to be easily deranged or broken, and in case of accident very soon put to rights or repaired. It is indispensable that it have also a means of registering the rate of the current at any point, during the whole of any given period; and this, when from immersion, or other reasons, the actual observation of the instrument is inconvenient or impossible.

These conditions, at least, are satisfied in Mr. Saxton's meter; and by it, the velocity of a current at any part of the surface or bottom of a river, and in all lines between them, can be easily and accurately ascertained; and, of course, when observations have been made in a sufficient number of lines, the mean velocity of the whole river, &c., at the place of observation, may be obtained. This, if multiplied by the sectional area of the river, would give the total volume of water passing during the time of observation. In cases where great accuracy is required, or the rate of flowing is very variable, any number of instruments may be simultaneously employed.

The instrument in question, consists of a revolving vane, a register, a tail, and a staff.

The staff is a rod of about seven-eighths diameter, generally six feet long, graduated into feet, inches, and tenths. On this staff the other parts of the instrument slide, and can be clamped at any point. The tail is a thin plate of metal, which may be from six inches to twenty inches in length, at pleasure; this is acted upon by the passing water, and preserves the axis of the revolving vane parallel to the direction of the current. The revolving vane is simply one or more arms attached to an axis; the surface of these arms is twisted, so that in parting from the axis it makes a continually increasing angle with it.

This angle is such that the action of the current upon an arm may, *in all velocities*, produces one revolution in some given length, as, for

In the adjoining Diagrams

A is the revolving vane.
B ... register.
C ... staff.
D ... tail.
E ... axis of the vane.
F ... frame.
G ... endless screw.
H ... register-wheel.
I ... front carrying-bar of H.
J ... rear ditto.
K ... detent.
L ... pivots of I and J.
M ... stud moving in the slot marked by the dotted line.
N ... string going up to the observer.
O ... eye of the bar I.
P ... spring which disengages H from I, and forces it down upon K.
Q ... joint.
R ... arm.
S ... staff-socket.
T ... conical screw.
U ... slits in T.
V ... milled-nut of T.
W ... milled-nut of E.
X ... milled-screw of D.
Y ... spring of K.

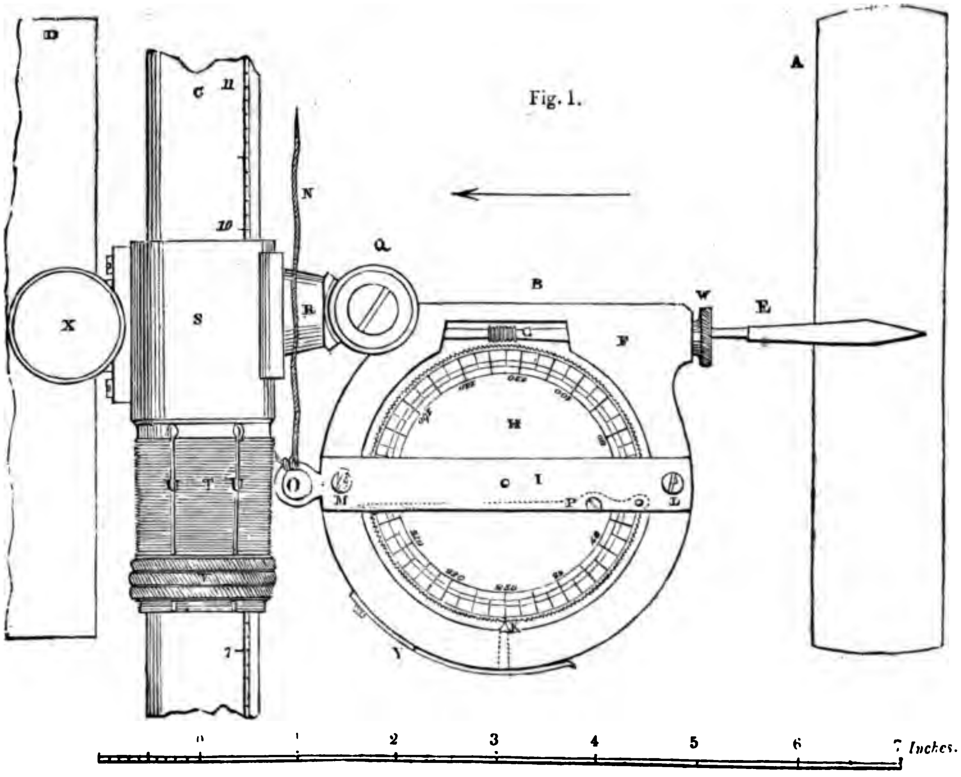


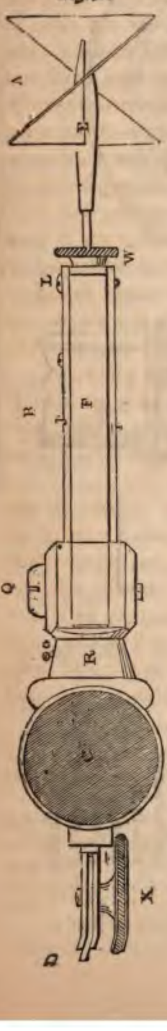
Fig. 1.

instance, in a yard, a foot, &c. Where this is accurately obtained, two consequences follow; namely, a particle of water, impinging on whatever part of the curved surface it may, will constantly exert the same effort to turn the axis, and it will pass across the whole breadth of the vane in the same time, whether its transit be near the axis, where the surface is little inclined and narrower, or near the extremity, where it is broader and the inclination is greatest.

The register consists of a wheel graduated upon its side, for the purpose of exhibiting the number of revolutions which may be made by the vane, when it is geared with it. This gearing takes place and terminates instantaneously, at the discretion of the observer. At all other times the register is ungeared and immoveable, whatever may be the rotation of the vane, or the position or motion of the instrument.

Fig. 1. *Side elevation of the register, vane, and part of the staff and tail.*

Fig. 2.



The axis (E) of the vane (A) is carried by a frame (F), and has upon it an endless screw (G). In this frame hangs the wheel (H) of the register, finely toothed on its edge, and graduated on its face; its axis rests on two carrying-bars (I J), the front one (I) only of which can be seen in this figure. From the lower part of the frame a detent (K) projects, and holds the wheel immoveable so long as it is engaged with it. The carrying-bars are attached to the frame at one end only, and they there move on a pivot (L); their angular motion is limited by a stop (M) at the other end, which moves in a slot, shown by dotted lines, in the frame: this slot just permits the wheel-teeth to enter the endless screw (G), high enough for gearing without jamming. The moveable ends of the carrying-bars are raised by a cord (N) attached to an eye (O), and thence going up into the hand of the observer; when this cord is not acting, the bars remain depressed, the wheel is freed from the endless screw, and forced upon the detent (K) by a spring (P), which lies between the front carrying-bar and the wheel. A joint (Q) at the connexion of the register with an arm (R), extending from the staff-socket (S), permits the axis of the vane to be inclined to the staff (C). This is sometimes useful in using the meter on the surface of currents. The staff-socket (S) has its lower part cut into a conical screw (T), with slits (U); a milled-nut (V) works on this, and clamps the socket on the staff (C). Another milled-nut (X) on the vane-axis (E) permits it to be taken out to be wiped or cleaned. The tail removes for packing up, by unscrewing the milled-screw (T), which clamps it when in use.

Fig. 2. *Plan of the vane, register, staff, and part of the tail.*—In this figure the rear carrying-bar (J) may be seen.

Fig. 3.

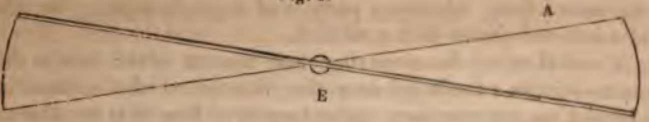


Fig. 3. *Front elevation of the vane.*

In using the meter, the zero end of the staff is generally set upon the bottom of the river, &c., and the vane is clamped upon the staff at the height of the line above the bottom in which the velocity of the current is desired to be known. When the staff rests upon the proposed point, and is held upright and free, the tail immediately swings down the current, and presents the vane full to it. The current sets the vane in action, but as the apparatus is now in the state represented in fig. 1., the vane is unconnected with the register, and the latter is therefore fixed and at rest. When it is certain that the vane has acquired a uniform velocity, and the observers are ready, time may be called, the string is at the same instant pulled up, the wheel of the register is geared with the vane-axis, and the vane and register now both move together. At the expiration of the time, the string is let go: the spring attached to the carrying-bar then draws it downward, and disengaging it from the vane-axis, gears it into the detent. This holds it steady, and though the vane still continues to run, it has no further effect upon the register. The latter may therefore be conveniently and leisurely examined, and by this means the number of revolutions made by the vane, (and, consequently, the velocity and the passing volume of the river,) may be ascertained with great precision.

When it is wished that the register should commence at zero, it may be done by depressing the spring (Y), which carries the detent; this detaches it from the register-wheel (H), and leaves it quite free. The zero upon it may then be set to the detent point as an index.

As by neglect after exposure, &c., the amount of the friction in the axis, and of the instrument may be different from that which existed at the time of the graduation, the meter should, previous to experiments where great accuracy is required, be carefully examined, and the effect of the actual friction be ascertained, by drawing the instrument with any convenient velocity through some known length of still water, and observing whether the register give this length exactly or not*. If there be any alteration, the difference will be discovered by this means, and should be allowed for in any immediate subsequent experiments.

To obtain portability, the tail and the vane are made to be easily separated from the staff-socket and register. The whole of the apparatus

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* The longitudinal canal in the Gallery of Practical Science has been used for this purpose. A length of sixty feet was marked off, and the meter drawn through the water, care being taken that the latter was always in a quiescent state. This was done many times with very varied velocities, and, whatever these happened to be, the length passed through by the meter was always indicated on the register, with almost mathematical precision.

(except the staff) can then be packed in a box, whose external dimensions are 6½ inches long, 4½ inches wide, and 1½ inch high. The staff also divides into two pieces of a yard each.

The surface of the revolving vane may be found in the following manner:—Given A the length in passing through which the vane is to make one revolution, say one foot; B the length of the arm, say three inches; C the breadth of the arm at the axis—this, for convenience, should be an aliquot part of A; say one-twelfth of an inch.

Describe the plan (fig. 4) and the elevation (fig. 5) of a polygonal prism, whose number of sides = the number of times that C is contained in A, (in this case, 12,) and whose radius = B.

Fig. 4.

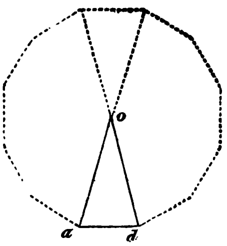
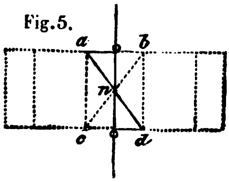


Fig. 5.



In fig. 4, let *ad* be one of the sides, and *o* the centre, join *ao* and *do*, then will *ao* be one edge of the arm, and *do* the other.

In fig. 5, let *abcd* be one of the sides, and *ono* the axis, of the prism, join *ad*, then will *ad* be the inclined line of the outward extremity of the arm. If the prism be supposed to be of some soft material, a cutting-line entering at *ad* and following the lines *ao*, *do*, will describe the surface required; it will change its direction as it proceeds, and become vertical as *ono* when it arrives at the centre.

In fig. 4, the surface of the arm will appear bounded by the triangle *aod*. In fig. 5, its twist will be understood, if the line *ad* is supposed to turn upon its centre until it coincides with *ono*.

THE GALLERY OF PRACTICAL SCIENCE.

No. I.

1. Standard Clock, deposited in the Gallery.
2. Circular of the Society.

Standard Clock.—This time-piece was completed by Messrs. Arnold and Dent, of the Strand, London, early in the year 1832. It was intended by them to be as perfect an instrument as the advanced state of the art of clock making, and their ample means and experience could enable them to produce. On its completion they believed they had succeeded in their aim, and they proposed to submit it to the most severe trial of its merits that could be obtained. A representation of the case was in consequence made to the Lords of the Admiralty, who, from the reputation of the makers, the introduction of some peculiarities in the mechanism, and the reported excellence of the clock, were of opinion that a trial might lead to results of public interest, and their Lordships gave directions that it should be made. The clock was, therefore, placed under the care of Mr. Pond, the Astronomer Royal, in the Observatory at Greenwich.

The assistants of this establishment made a series of uninterrupted observations on the going of the clock, from the 20th of March, 1832, to the 26th of April, 1833, inclusive, and registered the results. They firstly, compared the time given by it daily, with that of the best clock in the Observatory; and secondly, as opportunity offered, with the time obtained by observations of the sun's transit, and they noted the state of the barometer and thermometer at the times of observation. As we believe the performance of this clock in keeping accurate time is unexampled, we have obtained permission to annex a copy of this register. On examining the column of the rate ascertained by the sun's transit, it will be seen, that during the whole thirteen months, the greatest difference between any two observations was 1·1 inch.

Tables of the observations made and registered at the Royal Observatory, Greenwich, in the years 1832 and 1833, under the direction of the Astronomer Royal, and certified by him, during a thirteen months' trial, of a clock constructed by Messrs. Arnold and Dent, of London, (Gravity-escapement.)

Explanation of the Arrangement.

Col. 1. The Day of the Month.

2. The Daily rate of the Clock (sidereal Time), as found by comparison with the Observatory Clock.

3. The Daily rate of the Clock (sidereal Time) as found by Observation of the Sun's transit.

4. Height of the Barometer at Noon.

5. Height of the Standard in-door Thermometer, kept in the neighbourhood of the Clock, at Noon.

M.DCCC.XXXII.

Day of Month.	MARCH.				APRIL.				MAY.				JUNE.				JULY.				Day of Month.
	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	
	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	
1	...	...	...	...	−0·3	...	...	...	−0·2	...	29·24	48°	−0·4	...	29·52	55°	...	...	...	...	1
2	...	...	...	...	−0·3	...	30·04	48°	−0·2	−0·2	29·24	54	−0·2	...	29·76	58	...	...	...	...	2
3	...	...	...	...	−0·3	...	30·33	...	−0·3	...	29·32	53	−0·2	...	29·59	58	...	...	...	...	3
4	...	...	...	...	−0·0	...	30·47	53	−0·3	...	29·67	51	−0·1	...	29·45	57	...	...	...	...	4
5	...	...	...	...	−0·1	...	30·37	55	−0·3	...	29·98	51	−0·1	...	29·45	60	...	...	...	...	5
6	...	...	...	...	+0·1	...	30·30	50	−0·2	...	29·96	56	−0·2	...	29·37	57	...	...	...	...	6
7	...	...	...	...	+0·1	−0·3	30·14	45	−0·1	−0·2	29·73	57	−0·0	...	29·50	57	+0·1	...	29·71	62°	7
8	...	...	...	...	...	...	30·05	48	+0·1	...	29·92	57	−0·0	−0·1	29·62	57	0·0	...	29·81	64	8
9	...	...	...	...	0·0	...	30·12	47	+0·2	...	30·17	48	+0·1	...	29·74	60	0·0	...	29·90	65	9
10	...	...	...	...	+0·1	...	30·10	46	+0·6	...	30·28	46	+0·1	...	...	58	−0·1	...	29·87	65	10
11	...	...	...	...	+0·2	...	30·04	50	+0·5	...	30·14	48	−0·1	...	29·66	58	−0·1	...	29·73	65	11
12	...	...	...	...	+0·6	...	29·84	46	+0·6	...	29·78	46	−0·1	...	29·46	61	0·0	...	29·79	64	12
13	...	...	...	...	+0·6	...	29·84	49	+0·6	...	29·57	46	−0·2	−0·1	29·44	64	+0·1	...	29·81	66	13
14	...	...	...	...	+0·1	...	30·06	51	+0·6	...	29·67	46	+0·3	...	29·62	62	+0·1	...	29·91	65	14
15	...	...	...	...	−0·2	...	30·00	48	+0·1	+0·4	29·68	47	+0·3	...	29·87	59	+0·2	+0·03	30·31	62	15
16	...	...	...	...	−0·2	+0·2	29·92	52	+0·1	...	29·73	47	0·0	...	30·00	60	+0·5	...	30·19	64	16
17	...	...	...	...	−0·1	...	29·91	48	+0·1	...	29·72	47	−0·5	...	30·01	61	+0·6	...	30·07	69	17
18	...	...	...	...	−0·1	...	29·60	56	+0·1	...	30·00	50	0·0	...	30·04	64	+0·6	...	29·99	62	18
19	...	...	...	...	−0·1	...	29·60	50	+0·3	...	30·12	52	0·0	...	30·00	65	+0·6	...	30·07	58	19
20	−0·0	...	29·52	45°	−0·1	...	29·61	49	+0·3	+0·1	30·08	56	+0·4	...	29·88	63	+0·6	...	30·09	58	20
21	−0·1	...	29·97	47	+0·3	−0·02	30·03	49	+0·1	...	30·12	54	+0·4	+0·3	29·83	61	+0·5	...	30·05	57	21
22	−0·1	...	30·05	47	−0·2	...	30·04	52	+0·1	...	30·12	58	+0·4	...	29·41	61	+0·3	...	30·11	57	22
23	−0·2	...	29·80	48	−0·3	...	29·68	53	+0·3	...	30·18	57	+0·5	...	29·72	63	+0·1	...	30·10	57	23
24	−0·1	−0·1	29·73	41	−0·1	...	29·63	51	+0·3	...	30·20	59	+0·4	...	29·87	62	+0·1	...	30·11	60	24
25	+0·1	...	30·06	42	0·0	...	29·78	48	+0·1	...	30·08	59	+0·3	...	29·93	59	+0·2	+0·4	30·11	61	25
26	+0·3	...	30·06	42	0·0	...	29·64	45	+0·1	...	29·90	62	+0·5	...	30·05	58	+0·3	...	30·07	62	26
27	+0·1	...	29·92	45	+0·1	...	29·76	47	+0·0	−0·04	29·90	57	+0·5	...	30·18	61	+0·3	...	...	...	27
28	0·0	...	29·96	45	+0·1	−0·1	29·60	45	−0·1	...	29·92	...	+0·2	...	30·30	62	+0·1	...	30·11	61	28
29	0·0	...	29·86	43	−0·1	...	29·41	48	+0·4	...	29·81	56	+0·2	+0·3	30·30	64	0·0	...	30·25	61	29
30	0·0	+0·1	29·86	49	−0·1	...	29·22	49	+0·4	...	29·78	...	+0·1	...	30·33	64	+0·1	...	30·27	62	30
31	0·2	...	29·67	44	...	...	...	...	−0·2	...	29·37	56	...	...	...	...	+0·3	...	...	62	31

Day of Month	AUGUST.				SEPTEMBER.				OCTOBER.				NOVEMBER.				DECEMBER.				Day of Month.
	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	Rate by Obsv. Clock.	Rate by Sun's Transit	Barom. at Noon.	Ther. at Noon.	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	Rate by Obsv. Clock.	Rate by Sun's Transit.	Barom. at Noon.	Ther. at Noon.	
	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	
1	+0·3	+0·2	29·97	62°	+0·6	...	29·54	60°	+0·3	+0·2	29·96	62°	+0·6	...	29·57	55°	+0·5	...	29·72	52°	1
2	0·0	...	29·76	63	+0·6	...	29·89	58	+0·3	...	29·83	61	+0·6	...	29·61	52	+0·4	...	29·59	52	2
3	+0·1	...	29·84	64	+0·6	...	30·10	58	+0·4	...	29·79	59	+0·6	...	29·68	53	+0·4	+0·5	29·40	46	3
4	+0·2	...	29·99	62	+0·6	...	30·17	62	+0·4	...	29·66	61	+0·6	...	29·74	50	+0·6	...	29·87	45	4
5	+0·2	...	29·86	62	+0·6	+0·6	29·99	58	+1·0	...	29·17	58	+0·6	...	29·65	42	+0·6	...	30·10	43	5
6	+0·2	...	29·81	64	+0·5	...	29·88	58	+1·1	...	29·43	57	+0·5	...	30·16	44	+0·6	...	29·92	44	6
7	+0·4	...	29·86	64	+0·5	...	29·75	59	+0·7	+0·7	29·53	56	+0·5	+0·6	30·27	44	+0·6	...	30·18	44	7
8	+0·5	+0·2	29·98	64	+0·5	...	29·95	60	+0·7	...	29·18	53	+0·7	...	29·86	44	+0·7	...	30·26	42	8
9	+0·3	...	29·99	69	+0·5	...	29·89	62	+0·7	...	29·83	54	+0·7	...	29·77	41	+0·5	...	30·30	45	9
10	+0·3	...	30·08	72	+0·5	...	29·68	59	+0·7	...	29·99	58	+0·7	...	29·54	43	+0·4	...	30·23	46	10
11	+0·3	...	30·23	71	+0·7	...	30·03	58	+0·7	...	30·22	61	+0·6	+0·8	29·50	46	+0·4	...	30·33	44	11
12	+0·2	+0·3	30·19	60	+0·7	...	30·22	57	+0·7	...	29·92	60	+0·6	...	29·61	43	+0·5	...	30·35	45	12
13	+0·1	...	29·83	64	+0·7	...	29·92	59	+0·7	+0·7	29·82	56	+0·5	...	29·67	42	+0·5	+0·6	30·07	45	13
14	+0·1	...	29·80	63	+0·7	+0·6	29·78	56	+0·7	...	30·22	52	+0·5	...	29·62	47	+0·6	...	29·86	44	14
15	0·0	...	29·80	65	+0·5	...	29·89	56	+0·7	...	30·13	...	+0·5	...	29·83	46	+0·6	...	29·52	45	15
16	−0·1	...	29·90	66	+0·4	...	...	...	+0·7	...	30·11	...	+0·5	...	30·19	46	+0·6	...	29·87	39	16
17	−0·1	...	30·03	64	+0·4	...	30·18	57	+0·7	+0·7	30·28	52	+0·5	...	30·28	45	+0·5	...	29·41	50	17
18	−0·1	...	29·80	65	+0·3	...	29·89	60	+0·6	...	30·17	53	+0·6	...	30·04	42	+0·5	+0.5	29·47	44	18
19	0·0	...	29·66	63	+0·3	...	30·26	53	+0·6	...	30·13	53	+0·6	...	29·81	45	+0·4	...	29·58	40	19
20	0·0	...	29·94	64	+0·3	+0·4	30·44	53	+0·6	...	30·23	48	+0·6	...	29·62	45	+0·4	...	29·85	37	20
21	0·0	...	29·81	64	+0·2	...	30·44	53	+0·6	...	30·25	50	+0·6	+0·5	29·47	46	+0·4	...	29·44	42	21
22	0·0	...	29·64	64	+0·1	...	30·29	57	+0·6	+0·7	30·15	49	+0·6	...	29·71	45	+0·4	+0.4	29·75	46	22
23	0.2	−0·1	29·82	62	+0·0	...	30·25	56	+0·7	...	30·24	49	+0·5	...	29·84	46	+0·2	...	29·56	47	23
24	+0·3	...	29·96	60	0·0	...	30·29	59	+0·7	...	30·26	50	+0·5	...	29·90	49	+0·1	...	29·90	44	24
25	...	...	29.70	60	+0·1	...	30·33	63	+0·8	...	30·32	48	+0·6	...	29·76	49	+0·1	...	...	...	25
26	...	...	29·60	61	+0·1	...	30·20	62	+0·8	...	30·28	49	+0·6	+0·6	29·46	47	+0·1	+0·1	30·04	42	26
27	+0·5	...	29·56	59	+0·2	+0·1	30·06	63	+0·8	...	30·26	47	+0·6	...	29·55	44	+0·2	...	30·08	41	27
28	+0·5	...	29·07	59	+0·2	...	29·98	60	+0·8	...	30·13	49	+0·6	...	29·58	43	+0·3	...	29·95	35	28
29	+0·5	...	29·25	56	+0·2	...	29·91	61	+0·8	...	29·82	51	+0·6	...	29·51	43	+0·3	...	29·53	39	29
30	+0·5	...	29·45	57	+0·2	...	29·86	62	+0·8	...	...	49	+0·6	...	29·73	42	+0·3	...	30·00	...	30
31	+0·5	...	...	58	...	...	...	...	+0·6	+0·7	...	51	...	...	...	...	+0·3	...	29·74	37	31

Day of Month.	JANUARY.				FEBRUARY.				MARCH.				APRIL.				Day of Month.
	Rate		Barom. at Noon.	Ther. at Noon.	Rate		Barom. at Noon.	Ther. at Noon.	Rate		Barom. at Noon.	Ther. at Noon.	Rate		Barom. at Noon.	Ther. at Noon.	
	by Obsv. Clock.	by Sun's Transit.			by Obsv. Clock.	by Sun's Transit.			by Obsv. Clock.	by Sun's Transit.			by Obsv. Clock.	by Sun's Transit.			
	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	Seconds	Seconds	Inches.	Fahr.	
1	+0·3	...	30·32	37°	+0·1	...	29·42	36°	+0·3	...	28·96	41°	0·0	...	28·92	...	1
2	+0·2	...	29·93	42	+0·2	...	28·76	44	+0·3	...	29·67	43	0·0	...	28·96	47°	2
3	+0·2	...	30·36	43	+0·2	...	29·08	...	+0·3	...	29·66	45	0·0	...	29·45	48	3
4	+0·2	...	30·43	36	+0·3	...	29·60	44	+0·3	...	29·63	45	0·0	...	29·39	48	4
5	+0·2	+0·2	30·22	36	+0·3	...	29·80	52	+0·3	+0·3	29·93	44	0·0	+0·0	29·76	47	5
6	+0·2	...	...	...	+0·2	...	29·90	48	+0·4	...	30·02	43	+0·1	...	29·87	48	6
7	+0·3	...	30·47	35	+0·3	+0·3	29·79	48	+0·4	...	30·18	40	+0·2	+0·2	29·72	47	7
8	+0·3	...	30·58	35	+0·3	...	29·63	47	+0·5	...	30·17	37	+0·1	+0·1	29·87	47	8
9	+0·3	...	30·46	35	+0·3	...	29·60	45	+0·5	...	29·99	35	+0·4	...	29·95	48	9
10	+0·3	...	30·10	32	+0·3	...	29·21	...	+0·5	...	29·83	38	+0·4	...	29·67	50	10
11	+0·3	...	29·75	33	+0·3	...	29·33	49	+0·5	...	29·94	37	+0·4	...	29·24	47	11
12	+0·3	+0·3	29·75	35	+0·3	...	29·53	...	+0·5	...	29·90	36	+0·4	...	29·26	45	12
13	+0·3	...	29·82	36	+0·4	...	29·30	48	+0·5	...	29·55	37	+0·3	...	29·62	44	13
14	+0·3	...	30·11	39	+0·4	...	29·30	45	+0·5	+0·5	29·21	34	+0·3	+0·3	29·32	...	14
15	+0·3	...	30·05	39	+0·4	+0·4	29·18	43	+0·1	...	29·31	37	+0·1	...	29·31	45	15
16	+0·3	...	30·07	38	+0·3	...	29·56	40	+0·0	+0·1	29·38	41	0·0	...	29·33	43	16
17	+0·4	...	29·99	38	+0·3	...	...	...	−0·0	...	...	...	0·0	...	29·44	...	17
18	+0·4	...	29·99	38	+0·3	...	29·45	44	−0·0	...	29·61	40	0·0	...	29·58	42	18
19	+0·4	...	30·15	36	+0·3	...	29·76	42	−0·1	...	29·86	39	0·0	...	29·89	42	19
20	+0·4	...	...	36	+0·3	...	29·85	42	−0·1	...	29·97	38	−0·1	− 0·1	29·98	45	20
21	+0·4	+0·4	30·19	33	+0·3	...	29·74	43	−0·1	+0·1	29·72	36	−0·2	...	...	46	21
22	+0·5	...	30·27	32	+0·3	...	29·91	41	+0·0	...	29·81	38	−0·2	...	30·09	...	22
23	+0·5	+0·5	30·46	31	+0·3	+0·3	29·72	41	+0·0	...	29·79	37	−0·2	...	30·08	49	23
24	+0·3	...	30·30	33	+0·3	...	29·36	39	+0·0	...	29·80	36	−0·2	...	29·89	49	24
25	+0·3	...	30·06	32	+0·3	...	29·48	...	+0·1	...	29·69	38	−0·2	...	30·14	50	25
26	+0·3	...	30·00	34	+0·3	...	29·13	45	+0·1	...	29·77	38	−0·2	−0·2	30·12	49	26
27	+0·2	...	30,04	37	+0·3	...	28·98	...	+0·1	...	29·97	38	...	...	...	52	27
28	+0·2	+0·2	29·81	41	+0·3	...	29·05	...	+0·1	+0·1	29·88	42	...	...	...	55	28
29	+0·2	...	29·24	39	...	...	...	...	+0·2	...	29·76	41	...	...	...	...	29
30	+0·1	...	29·50	38	...	...	...	...	+0·2	+0·2	29·61	42	...	...	...	50	30
31	+0·1	...	29·57	35	...	...	...	...	+0·1	...	29·64	44	...	...	...	49	31

(Signed) J. POND, *18th Dec.*, 1833.

Dr. Robinson, of Armagh, and some other astronomers, have thought that the temperature and pressure of the atmosphere interfere with the accurate going of a clock, in spite of all the skill and precautions of the mechanician. There are others who maintain an opposite opinion. Both parties have, in the register annexed, some data for investigating the subject, upon which they may depend.

The deposit of a clock of such high reputation in the gallery must be peculiarly acceptable to visiters, who, from a love of accuracy, or for scientific purposes, may wish to obtain the true time, or to compare their watches or chronometers with a correct standard. That no doubt may ever arise to disturb the confidence which is invited to be placed in this clock, the council have arranged with the makers, that it shall always remain under the superintendence of the latter entirely. No person unauthorized by them can wind it up, nor set it, nor, indeed, get at any part of the interior. It may, therefore, be depended upon as a standard measurer of time.

As this clock was originally intended to be used for astronomical purposes, *i.e.*, to indicate the peculiar time and notation of the hours which are used in observatories, the circles upon the dial were arranged differently from those of ordinary clocks, principally in order that each hand may have its own centre of motion, and the hour-circle was numbered from 1 to 24, so that the hour-hand made but one revolution in a day of twenty-four hours. This continuation of the hours in one series, is far more rational than the one in ordinary use, which, by dividing it into two series, gives two XIIs, two Is, &c., in every daily period; often producing ambiguity and doubt, counterbalanced by no advantages whatever. But still, as we do not yet, like the Venetians, use the twenty-four hour series, a new hour-circle has been engraven for this clock and though the hour-hand still goes round but once, instead of twice as in the common clocks, in twenty-four hours, yet as the circle is divided into two series, and the words "MORNING—NOON—EVENING—NIGHT" are placed near the hours which occur in those portions of the day, no difficulty will be found in reading off the true time from this dial.

Before we leave the dial, we wish to direct attention to the seconds'-hand, viz., that which is in the uppermost of the two small circles. It is very remarkable for the quietness of its beat, for its great velocity when in motion, and for its perfect, and comparatively long, repose when the beat has been made. The beat is a fine example of a perfectly dead one, by which is meant, that when the second is struck, there is not the slightest recoil of the hand, but that it becomes, and remains, entirely motionless until the time arrives for the succeeding movement.

Looking into the body of the clock-case, we perceive the pendulum and the weights. In the latter there is nothing remarkable; they are wound up weekly. But the immense importance in practical science of the pendulum has long made its laws and application subjects of the most intense study to the philosopher and the mechanician. Volumes of computations have been made of its motions; interminable discussions have taken place as to the curves it describes and ought to describe; and innumerable designs for its construction and application, have been the consequence. In all probability these are after all but the introduction to further investigations, and a more refined and extensive application of this invaluable instrument.

In a clock the pendulum is used as *the regulating power*, and is so called. The weights and train are *the maintaining power*. Each alone would be useless; combined, they produce an instrument of the most extensive utility. *With an unconnected* pendulum, delicately suspended, we might measure a few seconds with tolerable accuracy; with the train and all its old regulators, little could be done, in point of accuracy, for a length of time; but if we combine the two, so that the latter shall communicate a constantly equal push to the former, and the pendulum shall successively interpose a constant and equal retardation to the motion of the train, we shall have the arrangement presented by a perfect clock. That the weight may not descend too rapidly, or unequally, a check is communicated regularly through the train, by the return-swing of the pendulum. That the arc described by the pendulum may not diminish in its range, the train transfers periodically a pressure from the weight. The production and perfect adjustment of this alternation of check and impulse is the aim of the skilful clockmaker; and it is in order to obtain it amidst the difficulties arising from the imperfection of materials, and the incessant changes produced in them by such subtle agencies as heat, magnetism, and electricity, that contrivances are in the course of almost daily invention. But so numerous, and so invincible are some of the difficulties, that among hundreds of schemes for connecting the vibratory motion of the pendulum with the rotary motion of the train, (an arrangement technically called *the escapement*,) there is scarcely any one in use in England, but that invented many years ago by the justly celebrated Graham; and of the many means which have been suggested to counteract the effects of temperature upon the pendulum, the compensation-pendulum invented by the same artist, has still the preference in all good clocks.

The escapement, however, in this clock, is different from Graham's, and from that and its peculiar ingenuity, is remarkable.

In all the best clock-escapements hitherto made, including those by Graham, the action of the train upon the pendulum is that of pressure, which continues during a certain time, and as the train and pendulum are in connexion for the whole of this time, the latter is exposed for just so long to any irregularity that may occur in the going of the train. In this clock *there is never any contact or connexion between the train and the pendulum*, the escapement-wheel of the former lifting up an intermediate agent, viz., a metallic sphere, which, when let fall, presses with its own weight only upon the pendulum in its descent, and gives it the proper impulse. It is from this mode of action that it has been called the *gravity-escapement*. It is scarcely possible to imagine a more elegant and perfect mode of administering a precise dose of effect. For its eminent success in practice, which is the great test of excellence in works of this nature, we again refer to the observatory-register of its performance.

The great object of the compensating-apparatus, which is seen appended to the lower end of the pendulum, is to counteract the effects of temperature upon the rod of the pendulum. If this rod remained always of an invariable length, there would be no necessity for compensation; but, let it be made of whatever material it may, it cannot escape the usual effects of heat and cold, and as it is surrounded by the atmosphere, it sympathizes with all its vicissitudes of temperature. These are so influential, that a difference of 15° Fahr. will produce a variation in a well-made pendulum-clock, not compensated, of 4 sec. per day. The *impossibility of* correcting this by observation as it occurs, is obvious.

A means of compensation which should simultaneously act with, and correct the effect of, the disturbing cause, became a necessary addition to every good clock. Happily this has been obtained, by taking advantage of an effect produced by heat upon different metals, viz. that of expanding each metal in a different degree.

If, therefore, bars of different metals are attached to each other, so that they expand in opposite directions, it is not difficult to imagine that their lengths may be so adjusted, that a point, at a certain distance from the end of one of them, may, by their opposite and unequal dilatation, be constantly preserved at the same distance. Upon this principle are compensation-pendulums constructed; they always preserve a certain length (or more correctly, a certain distance between the point of suspension and the centre of oscillation)*. In such pendulums, any injurious elongation downwards, is neutralized by a proportionate elongation upwards of the compensating part, so preserving the centre of oscillation always at the same level. In the pendulum of the clock under consideration, the metals employed are steel and mercury.

Now, as this pendulum is designed to beat seconds at London, it has a length of 39·2 inches. The height of the compensating column of mercury is 6·65 inches. If the temperature were suddenly elevated in the clock-case, and we suppose it to affect the steel rod only, that rod only would elongate, the centre of oscillation would descend, and the clock would, if the elevation were continued, go slower; if it affected the mercurial column only, it would elongate, and the centre of oscillation rise, the clock's rate, therefore, would, upon the centre of oscillation, be accelerated; but as this effect of temperature is experienced by both, and the effect of the descent of one of them is just balanced by the effect of the rise of the other, the centre remains, during these changes, steady at the same level; and the going of the clock remains unaffected. This kind of compensation-pendulum is the one invented by Graham, and is called the *mercurial* one: one of its peculiarities is the facility with which it may be accurately adjusted.

The whole is a beautiful instance of the advantages which varied knowledge may confer on its possessors. A clockmaker could hardly long be ignorant that metals may be expanded by heat; but, unless he was an accurate observer, he might have passed a life without being aware of their unequal expansion. With such a man, the idea of a compensation-pendulum of this nature could never have originated.

By the index on the lower end of the pendulum, and the graduated arc over which it plays, the range of the swing may be observed. This is useful to indicate any variation which may take place in the strength of the maintaining power. When the clock has been recently oiled, and is in high condition, the index generally travels over two of the larger divisions; these are degrees.

The graduated cone and index, at the bottom of the rod-part of the pendulum, is for adjusting it, so that it may beat the time required.

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* The *centre of oscillation*, is "that point in a vibrating body into which, if the whole were concentrated and attached to the same axis of the sun, it would then vibrate in the same time the body does in its natural state."—GREGORY.

As one of the objects of the proprietors and of the makers, in placing a clock in the Gallery, has been to furnish a correct standard of time, free from nearly all the sources of error to which public clocks are liable, it will be necessary to say a few words as to its use. The time it indicates is *mean solar time*, or that which all good clocks (not astronomical ones) should show. No allowance for equation, or anything else, is necessary. It gives the precise time at the Gallery, and for all places under the same meridian, *but for no other*. This must be kept in mind by all those who, not having acquired much astronomical knowledge, and perhaps possessing a good watch, may become hypercritical observers of time by the facility with which they now may adjust their own chronometers. Travelling due north and south, with their time-keepers set by this clock, they may, if they have good ones, set up for oracles on the subject, by merely comparing others with their own; travelling east and west, they must, however, make an allowance, which becomes appreciable in a very short distance. Setting their time-piece at this clock, and going to its makers, at 84, Strand, they will find, on comparing it with the regulator there, that it is nearly 1 sec. too slow; if they proceed east and compare it with the clock in the ante-room of the Royal Society, at Somerset House, it will be nearly 2 sec. slow; going on to the Royal Exchange, it will be still more so, about 10 sec.; and if they reach the Royal Observatory, it will differ about 30 sec., or half a minute, from the mean time there.

This variation might annoy an inexperienced observer, and his chronometer might suffer in his opinion; he would be wrong if it did, for such an exact variation would be a proof of very rare excellence: the reason will be evident if such a person will reflect that as the noon twelve o'clock of each place, is the moment when the sun is on the meridian of that place, no two places lying east and west of each other can have the same time, and the difference will be the time that the sun is apparently travelling from the meridian of the eastern place to that of the western one. This, supposing the Royal Observatory and the Adelaide Street Gallery to be the two points in question, is, as has been said, about thirty seconds, and so long is the sun apparently travelling from one place to the other. If the observer, after the comparison at the Royal Observatory, should return to the Gallery without altering his watch, he will find it again exactly correct. Let him proceed west, he will now no longer be surprised that he is about 2 sec. faster than St. James's Church clock if it be right; or that he will be as much as nearly 17 sec. fast, if he visit the Observatory of Sir James South, at Kensington. If then he add this new difference to that he found between the Adelaide Street Gallery and the Observatory in Greenwich Park, he will find that there ought to be, and no doubt actually is, a difference of nearly 47 sec. between the two Observatories, the clock of Greenwich being, of course, so much faster than that of Kensington. One reflection suggests itself from this subject. Supposing all the church clocks, nearly in a line due east and west of St. Paul's, kept exact time, and were accurately adjusted, an observer on the gallery which crowns the dome of that edifice, might, in a still night, hear the stroke of *Limehouse church clock*, and as all the intermediate towers between it and

Kensington struck out in succession, he would witness a most impressive mode of marking the rate at which London and its slumbering millions were silently and continually whirled round the axis of the globe.

Near the clock is suspended a table containing a few places east and west of the Gallery, with the difference in time which a good watch should show at each, if set by the standard clock in the Gallery. It is the intention of Mr. Dent to extend this list, by collecting all such points in London and the neighbourhood, whose longitude and latitude have been ascertained correctly; and to lay them down, and present them in a chart with the mean solar time due to each*. A chart of this nature, containing a large number of correct points well sprinkled over the metropolis and its environs, attached to an accurate time-measurer, of easy access, like the clock in question, will furnish a means of deciding in the most satisfactory manner, upon the several pretensions to superiority which are now made by the watchmakers of London. It will have a silent, but a most powerful effect, in producing excellence in time-keepers.

Circular of the Society for the Illustration and Encouragement of Practical Science to the Provincial Scientific Societies of Great Britain.

Extract from the Minutes of a recent Meeting of the Council.—

"That it is consonant with, and will promote the accomplishment of, the objects of this Institution, to establish a communication with the various scientific Societies of the principal Towns of the Empire.

"That, for this purpose, such Societies be invited to make known to the Secretary of this Institution the name of the respective persons in London who supply such Societies with Books, that this Institution may be enabled to send their Catalogues, free of expense, to such Societies, which can be accomplished by enclosing them in the Booksellers' parcels.

"That such Societies, when they shall be fully informed of the objects of this Institution, the consequence into which it has grown, and its public importance, be invited to contribute, through their Members and friends, to its yet further advancement, by forwarding such works of practical science, art or virtù, as may be deemed worthy of public exhibition in the Metropolis.

"That the Presidents and Secretaries of all such Societies, shall, upon application to the Secretary of the Society, receive a free ticket of admission to the Gallery, during their occasional visits to London.

"That such Societies be further invited to offer any suggestions which the local knowledge of their Members may enable them to do with advantage to the general objects of this Institution."

Adelaide Street Gallery, Feb., 1836.

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* Communications, post paid, of the names of any places, with their longitude and latitude, and the authority and calculation given, will be received for this purpose at the Gallery.

REVIEW.

I. *The Doctrine of Proportion, or Geometrical Admeasurement by Similar Triangles, practically applied to Expanding or Diminishing Drawings.* London, Ackermann, 1836.

We are sorry we cannot say anything in commendation of this work, notwithstanding its gay and prepossessing exterior; but the author is obviously little conversant with geometrical, or indeed with drawing of any kind.

Most of our readers are aware that artists and draughtsmen employ a method for copying drawings, technically termed *squaring;* that is, they cover the surface of the original with a net-work of *rectangles*, and, having constructed another set of *similar* figures in the requisite proportion, according as the outline is to be reduced or enlarged, they trace in the corresponding spaces, the portion of the contour of the original design that falls within each rectangle. The advantages of this mode of proceeding are, that from the simplicity of the geometrical elements employed, the *right angle* and *parallel right lines*, the construction is easy, and admits of the utmost, attainable, practical accuracy.

The object of the work before us, is to recommend the substitution for this, of another equally well-known geometrical principle, namely, that of drawing right lines from any assumed point, through all the principal ones of the original outline, and then making the corresponding segments of the legs of an equal series of the angles thus formed, constructed on the copy, in the requisite proportion to those, intercepted between each point of the original and the assumed vertex. Now every *practical* geometrician is aware that, to construct a series of angles at a point equal to another set, is an extremely nice operation; and that unless it be done with great exactness, the copy obtained by means of them would be very inaccurate, more especially if the original drawing were to be enlarged. In addition to this fatal defect in principle, the proposed plan of proceeding is excessively complicated, and requires a great number of lines to be drawn, to the obvious injury of both copy and original; and further, the method is totally inapplicable to the copying of paintings or drawings, on the surfaces of which no such operations of any kind can be allowed. In these cases the artist either divides the edge of the frame into equal parts, and placing pins in the points of division, stretches threads across the face of the picture, to form the rectangles or *squares;* or he uses a light frame, which admits of this proceeding, and is then applied to the original picture.

The eighth plate in the work under notice is intended to illustrate what the author terms "a new rule in perspective;" to the discovery of which he, rather unluckily, urges his claim. We venture to assure him that it will never be disputed by any one conversant with the simplest elements of that branch of practical geometry; this rule being, in fact, false in principle, and worse than useless in practice. The author imagines that equal, equidistant, rectilinear figures, the planes of which are *not* parallel to that of the picture, are perspectively projected into *similar*[*] figures, decreasing in size in a simple geometrical ratio, as the originals recede from the point of sight. Now, as every one knows, this is not the fact; such projections being *similar* only in the case of the planes of the original, and that of projection being parallel. To expose the second and more general part of the error, let the reader take AB to be an original line, divided *equally* by the points CDE, &c.; let $abcde$, *&c.*, be the corresponding projections of the original points, and let V be the vanishing point. Then aV, cV, dV, - - - bV will form an *harmonical* progression, and not a simple geometrical one, as the author's construction would make them; or $aV : ac :: bV : bc$, and not $aV : cV :: cV : bV$, as he imagines.

But in short, any artist, who is a geometrician, has only to look at the plate in question, to see such an unfortunate complication of error, as will fully establish the truth of our first assertion. The examples given throughout the work, both as regards the taste of their selection and their execution, cannot fail to excite a smile.

Where did the author acquire such phraseology as "demonstrating" for determining; "point of occurse;" "proportionality," *cum multis aliis?* The language throughout is singularly incorrect.

II. *Artisans and Machinery: the moral and physical Condition of the Manufacturing Population considered with reference to Mechanical Substitutes for Human Labour.* By P. Gaskell, Esq., Surgeon. London, Parker. 1836.

Artisans and Machinery! the title is too mitigated. Had it been called, "MACHINERY AND ITS VICTIMS," it had been nearer the truth.

This is, indeed, a bold and original work. Its author has had the courage to oppose himself to prejudice and sophistry; to search for truth without regard to the claims or assumptions of national vanity. Readers and writers on the state of Great Britain, in reference to her commerce and manufactures, have been deluded and misled by the splendour of the mechanical and other scientific operations which distinguish the present day.

The "*Results* of Machinery" have been paraded through two hundred and odd pages, small, but closely packed,—of a work which assumes the benign and friendly title of the "Working-man's Companion." Its "Economy" has been displayed by Mr. Babbage, in a fairly sized duodecimo, consisting of a perfect galaxy of wonders. The power of science to supersede the human labourer is proudly emblazoned; and the useless operatives are bidden, to "become capitalists, and go out of the market;" and after all, only one side of the picture is properly displayed. "Cheap! Cheap!" is the cry; but there may come a time, when the bubble may be blown too tight. The Minerva of manufactures and commerce rejoices in the success of her system, and dazzles our eyes by the effulgence of her golden shield. This will one day, perhaps, be turned, and the reverse may then be discovered to be of baser metal.

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[*] Throughout this notice the word *similar* is used in its strict geometrical meaning—the angles *equal*, and the sides *proportional*.

"It is a new era," says Mr. Gaskell, "in the history of commerce that an active and increasing trade should be the index, not to the improvement of the condition of the working classes, but to their poverty and degradation: it is an era at which Great Britain has arrived; and it behoves every man, anxious for the well-being of his country, to turn his attention to this extraordinary fact."—(p. xi.)

"The enormous export trade which has grown up, has filled the minds of many people with the most extraordinary delusions. Let us, however, see how and upon what terms it has grown to its present magnitude.

"The terms *official* and *declared* value must be explained before we can make ourselves understood. *Official value* indicates *quantity* only: it is the quantity of any given export, reduced to money by a fixed and unvarying scale, adopted by the Custom House many years ago. Thus, in speaking of the *official* value of an exported article, we say, in 1800 it was 1,000,000*l.*; in 1835, 10,000,000*l.*; that is, a certain number of yards were valued in 1800 at one million, and another certain number of yards at ten millions in 1835, both upon the same scale; and this advance points out at once that *ten times* more yards were exported at the last period than the first.

"*Declared value*, on the contrary, is the *real price* of the exported article, according to the declaration of the exporter. This signifies, therefore, the absolute worth of the article; and hence, the *official value* and the *declared value*, when compared, show at a glance the increase or decrease in the worth of the article. If the *official value* rises, whilst the *declared* remains stationary or declines, it is obvious that a greater quantity of goods are disposed of, without any correspondent return in money.

"The condition of the export trade, connected with our cotton manufactures, is singularly instructive as to the effects of machinery upon production and value. In 1814, the *official value* of the cotton exports was 17,655,378*l.*; *the declared value*, 20,033,132*l.* In 1833, the *official value* of cotton exports was 46,337,210*l.*; the *declared value*, 18,459,000*l.*

"It is worth while to pause a moment, and reflect on this extraordinary statement, founded as it is on Parliamentary Papers and Finance Accounts. The clearest way of showing the depreciation in value to the non-commercial reader, is to call the pound, (money,) in *official* value, yards, when it will stand thus—

1814, sold 17,655,378 yards for £20,033,132.
1833, sold 46,337,210 yards for £18,459,000.

so that, notwithstanding we have almost trebled our export trade since 1814, its absolute return is nearly 2,000,000*l.* less in 1833 than 1814.

Who does not glory in the progress of science? Who would stint or stay the advance of human intellect? Would say to invention, "thus far, and no farther, shalt thou go." Viewing the action of "the Iron Man," as the *self-acting* mule is fitly called in Manchester by the operatives, —the wondrous invention of Roberts, "a machine apparently instinct with the thought, feeling, and tact, of the experienced workman; calculated to perfect the function of a finished and adult spinner; and to restore order among the industrious classes." Who does not feel a throb and glow of proud satisfaction at the victory obtained by thought and inventive skill? Seeing a single workman—attached to the gentle giant of steam—producing as much of a given article as two hundred and seventy could effect with the more ancient assistants of labour,—150,000 workmen wielding the force of 40,000,000*. Who would not rejoice at the leisure and enjoyment which must surely be the lot of the operatives, where such powers prevail? But is it so? The true answer, we fear, is given in the following table, inserted by Mr. Gaskell, p. 375.

" This table shows the average wages paid for weaving a six quarter sixty reed cambric, 120 picks in one inch, the average price of flour, meal, potatoes per load (240 lb. to the load), butcher's meat per lb., together with the average price of rents, paid for a four and two-loom dwelling-house, during the last thirty-eight years, in the borough of Bolton. This table is well deserving a very careful examination, as it shows at one view the elements of the domestic condition of the hand-loom weaver:—

Years.	Wages.			Flour per Load.			Meal per Load			Potatoes, per Load.		Butcher's Meat per lb.	Rent for Four Looms.	Rent for Two Looms.		
	£	s.	d.	£	s.	d.	£	s.	d.	s.	d.	d.	£	£	s.	d.
1797	1	9	0	2	7	0	1	19	0	6	3	5	8	5	10	0
1798	1	10	0	1	17	0	1	12	0	5	0	4½	8	5	10	0
1799	1	5	0	2	11	0	2	1	6	7	6	4½	8	5	10	0
1800	1	5	0	4	14	0	4	1	6	15	6	6	8	5	10	0
1801	1	5	0	5	4	0	3	12	0	6	0	7	8	5	10	0
1802	1	9	0	2	7	0	1	10	0	7	0	7	8	5	10	0
1803	1	4	0	2	8	0	1	11	0	8	0	6½	8	5	10	0
1804	1	4	0	2	11	6	1	18	0	8	0	6	9	6	0	0
1805	1	5	0	3	0	0	2	0	0	7	6	6	9	6	0	0
1806	1	2	0	3	0	0	2	0	0	8	0	6½	9	6	0	0
1807	0	18	0	2	15	0	2	3	0	8	0	7	9	6	0	0
1808	0	15	0	3	2	0	2	9	0	7	0	5	9	6	0	0
1809	0	16	0	3	10	0	2	6	0	7	0	7	9	6	0	0
1810	0	19	6	4	0	0	2	5	0	8	0	8	9	6	0	0
1811	0	14	0	3	10	0	2	5	0	8	0	7½	9	6	0	0
1812	0	14	0	4	2	6	3	15	0	14	0	8	9	6	0	0
1813	0	15	0	3	13	0	2	10	0	9	0	8	9	6	0	0
1814	1	4	0	2	8	0	1	16	0	8	6	8	9	6	0	0
1815	0	14	0	2	2	0	1	12	0	8	6	6½	9	6	0	0
1816	0	12	0	2	10	0	1	14	6	9	6	5	9	6	0	0
1817	0	9	0	2	5	0	1	10	0	9	6	4½	9	6	0	0
1818	0	9	0	3	0	0	2	10	0	7	6	5	7	5	10	0
1819	0	9	6	2	3	0	1	18	0	5	6	6	7	5	10	0
1820	0	9	0	2	5	0	1	14	0	7	3	5½	7	5	10	0
1821	0	8	6	1	18	0	1	7	0	6	6	5½	7	5	10	0
1822	0	8	6	1	14	0	1	6	0	5	0	4	7	5	10	0
1823	0	8	6	2	0	0	1	10	0	5	7	4½	7	5	10	0
1824	0	8	6	2	2	0	1	14	0	7	0	6	7	5	10	0
1825	0	8	6	2	7	0	1	16	0	7	6	5½	7	5	10	0
1826	0	7	0	2	12	0	2	4	0	7	0	4¾	7	5	10	0
1827	0	6	6	2	8	0	1	14	0	7	6	5	7	5	10	0
1828	0	6	0	2	6	6	1	12	0	5	6	4½	7	5	10	0
1829	0	5	6	2	2	0	1	12	6	6	0	5	7	5	10	0
1830	0	5	6	2	1	0	1	11	0	7	0	5½	7	5	10	0
1831	0	5	6	2	1	6	1	14	0	6	0	5	7	5	10	0
1832	0	5	6	1	15	0	1	4	0	4	3	4½	7	5	10	0
1833	0	5	6	1	14	6	1	4	6	4	7	4½	7	5	10	0
1834	0	5	6	1	14	6	1	4	0	6	0	4½	7	5	10	0

Have, therefore, increased powers, in fact, produced increased happiness, advanced intelligence, higher moral attainments? These are questions which are not usually agitated when artisans and machinery are the subject of discussion; and yet it is these points which ought to engage the attention, if the condition and prospects of the great majority of mankind be considered. These accordingly are the topics which form the staple of Mr. Gaskell's volume. Fully appreciating our national skill, and our national enterprise, he yet, with unsparing and unflinching hand, tears off the veil that covers the defects of our national system. Rejoicing in our means of producing universal, diffusive happiness, he essays to disabuse us of the fallacious conclusion that such happiness is produced. Not cheated by the beauty of the vase, he has carefully analyzed its contents. He has examined the sparkling draught, and having, as Shakspeare says, "seen the spider there," proclaims, in tones not loud and petulant, but deep, decided, and impressive,—the poison he has discovered.

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* *Artisans and Machinery*, p. 316.

It is in this manner that Mr. Gaskell intrepidly proceeds, dividing his work into a number of chapters: on Domestic Manufacture; the Factory System; Social Condition and Morals; Infant Labour; Female Labour; Physical Condition; Health; Education, Religion, Crime, Combination, Subjugation of Labourers, Influence of Machinery, Its Extent and ultimate Consequences. We cannot trace his course, but the details he presents are appalling yet incontrovertible, and may suffice to make the most rigid economist pause, before he proceeds further to urge forward his iron system. Will he, if he cannot deny the following terrific summary, have the nerve "to shut the gates of mercy on mankind?"

> "The first fact which meets us is—that the poor rates of the kingdom have risen, during the progress of mechanical adaptation to processes hitherto demanding human labour, to the enormous sum of nearly 8,000,000*l.* sterling per annum. The second fact is—that a tide of demoralization has swept over the land, displaying itself in the agricultural districts by incendiarism and other forms, the details of which have been rendered familiar to the public by the Report of the Poor Law Commission [*], and in the manufacturing districts, in the shapes we have already spoken of. The third fact is—that from the impossibility of finding adequate remuneration for labour, no less than 351,056 persons have left our shores for Canada between 1812 and 1832; and that from the 7th of May, 1833, to the 24th of September, 1834, upwards of 30,000 emigrants departed from the port of Liverpool alone. The fourth fact is—that there are one million of human beings dependent on hand-manufacture, who are literally starving in the midst of the magnificent edifices housing the steam-engine and its workers, without the slightest hope or chance of improving their industrial condition [†]. The fifth fact is—that two millions of hand-loom weavers in Hindostan have been driven from their labour by machinery here, multitudes of whom have perished by famine [‡]. The sixth fact is—that there are hundreds of thousands of domestic manufacturers connected with the bobbin-net, woollen, silk, flax, linen, and iron trades, now suffering extreme privations, and who will shortly be driven from their peculiar province of industry by competition with steam-production. The seventh fact is—that the absorption of the household manufacture of the kingdom into factories, has completely deranged the social system of our labouring community. The eighth fact is—that the breaking up of the indus-

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[*] *Vide* Report, *passim.*

[†] *Vide* History of the Cotton Manufacture, p. 239.

[‡] Minutes of Evidence. Select Committee on Hand-loom Weavers, p. 311.

trial occupations of the people has led to so much idleness and dissoluteness, that the legislature (overlooking the cause) has determined to cover the country with workhouses, a measure which takes us back two centuries in the career of civilization*. The ninth fact is—that crime has proceeded at a fearful pace; the commitments in 1811, (as early as data are in existence)† being 5,337, and in 1832, 20,829. The tenth fact is—that discontent, violence, and organized unions, threatening the very safety of manufactures, universally characterize the artisans of the present day. The eleventh fact is—that drunkenness‡ and irreligion have made fearful advances amongst the depressed operatives§."—(p. 323-24.)

For all these facts and assertions various authorities are quoted, and we cannot here help wishing that the author had in addition made use of Mr. Marshall's *Statistical Tables*, published about 1834, by order of the House of Commons. In that elaborate work, the pith and marrow of six-hundred volumes of Parliamentary Reports, digested with immense industry and consummate skill, into the form of tables, full confirmation of all these assertions is to be found; and the truth of our author's statements are most decisively and impregnably supported.

What then is our condition? We double, triple, increase tenfold, a hundredfold, our wealth, because we increase to such extent our productive powers; and yet we fling away the advantages which might be realized by running, madly, the heedless race of competition. We vaunt ourselves on our increased population, while their numbers, rendered superfluous by our false and absurd social arrangements, are driven with contumely to distant shores, in order to find the means of support; and yet we have, at home, a plethora of wealth, and space for double our present amount. We affect to wish for the improved condition of our operatives; and yet we strenuously urge forward the increase of foreign commerce, which can only be maintained against our competitors, by *keeping down* the rate of wages. We invent machines, whose beneficial action might confer on the highest conceivable numbers that could inhabit our island, the blessings of leisure, education, and physical comfort; and having thus exerted our creative powers, we start in horror, like Frankenstein, from the contemplation of the power to which we have given life and motion, and strive to render nugatory its benefits.

These stupendous errors, it is true, have not been deliberately committed; they have gradually arisen out of circumstances that, apparently, could not be controlled; but the time is fast approaching, when the further endurance of their effects will be impossible. Individuals, associations, and governments, have sedulously inquired the causes that could have originated such tremendous evils out of what ought to have produced unmixed good, and they have essayed to remedy the acknowledged evils, but in vain. Emigration societies, truck bills, factory bills, poor laws' amendment bills, are monuments at once of their good intentions, and of their feeble comprehension. All are slaves to the enormous fallacy, that, to whatever extent human labour be superseded, it must still be necessary to find employment,—*work*,—hired action,—for those who now form the class or caste of workers.

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* Poor Law Amendment Act.

† *Vide* Table, Progress of Crime, in Appendix.

‡ *Vide* Table of Spirits consumed in Great Britain, in Appendix.

§ *Vide* Minutes of Evidence before Committee on Hand-loom Weavers, *passim*.

An impression, among those who, in their speculations, are without the pale of stern political economy, is however gradually arising, that all is not right. Thus Mr. Babbage, who originally wrote his *Economy of Machinery*, apparently, to show the human labourer of how little value he was becoming, seems latterly to have discovered that something more was due to the millions, than to exhibit them as slaves to the units. Mr. Babbage, in his late edition, regrets that the working classes *do not* reap, from advancing science, the share of advantage that they might expect. His quick perception immediately detects the fact, that the capitalists, the masters, would not willingly lend a hand to the work of regeneration; but fearless of the sneer or the anger of an unreflecting world, he recommends that bands of working men, raising, by the exertion of prudence and economy, small capitals of twenty to forty pounds each, should club their total amount, and establish independent *joint-stock labour companies*, and thus realize to themselves not only *wages* but also *profits**.

There is, in all this, something bold and characteristic. It will be at once perceived, that it involves a complete change in our social arrangements; that it is, in fact, a direct and open attack on the supremacy of capital. And so, in the *Results of Machinery*, "Become capitalists, go out of the [labour] market." What do these Delphic hints, these oracular exhortations, mean, unless they declare a perception of the present unequal, imperfect, and prejudicial action of our immense scientific resources; and point to a great and powerful exhibition of the combinative principle, as the only means of giving to the labourer his fair proportion of advantage derivable from these resources. These two, the most lauded books on the subject of machinery, thus tacitly or openly presume or admit that, to the labourer, the powers of science, while held by individual capitalists, is erroneous and defective, in spite of the interest, the triumph, and the feverish excitement of competitive prosperity. Is another authority worth calling for? Take the *Edinburgh Review*†.

"Labour's thousand arms of sinew and of metal, all-conquering everywhere, from the tops of the mountains to the depths of the mine, and the caverns of the ocean, ply unceasingly for the service of man,—YET MAN REMAINS UNSERVED. He has subdued this planet, his habitation and residence, yet reaps no profit from the victory. . . . . . . Thus *change, or the irresistible approach of change, is manifest everywhere*."

Mr. Gaskell, then, does but comment fully and openly on the texts afforded by the writers we have quoted; but this commentary is not, with him, to be given by side-wind and implication. If the evil exist, it must be probed to the bottom, and to do this is the task the author places before himself. He examines his all-important subject, the anomalous and disastrous condition of the people, in every point of view. He glories in the perfection of science, but with unsparing hand exhibits its appalling affects upon the mass of mankind, in this highly civilized country. He has produced, say the quietistic economists, the utilitarians of *class and caste*, "a very clever BAD BOOK."—He needs no higher encomium. The man who endeavours to "see the end, from the beginning;" who will not join in the hush-cry of "peace and safety," when there is no peace; who disdains to minister to the vitiated cravings of avarice, vanity and selfishness, by striving to

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* *Economy of Machinery*, 1833, p. 253.

† *Edin. Review*, 1831; article on Schlegel and Hope.

> . . . . Skin and film the ulcerous place,
> Whilst rank corruption, mining all within,
> Infects unseen,

is ever considered troublesome and impertinent. This rule has been made absolute, in all cases, from Noah downwards. But the question is not whether his statements be pleasant and agreeable to the preconceived notions of fashion and theory, but whether they be *true*. To us it appears that they cannot be impeached. His references for verification are perpetually made, and to the best possible authorities; made to the *Reports* of the Poor Law Commissioners; to the works of Dr. M'Culloch and Mr. Babbage; to Baines's *History of the Cotton Trade;* to Dr. Ure's *Philosophy of Manufactures*, &c., so that he leaves scarcely any point resting on his own unassisted dictum.

The old proverb says "one mend-fault is worth two find-faults." This is only half true. The fault must be discovered before it can be mended. The vices of our present system afford ample materials for the volume before us. It is too late to close our eyes to facts which such concurrent testimonies combine to establish. Let those, then, who discern the remedy, present it clearly, explicitly, and perspicuously, to the public eye. The door of inquiry is flung open; it must not be shut again.

III. *Three Addresses delivered before the Society for the Promotion of Science and Literature of Staines and its Vicinity*, by the Rev. Robert Jones, D.D., M.R.S.L., Vicar of Bedfont. Staines: Smith.

This little series may be considered a manual to be used on all future occasions when similar Societies may be suggested, formed, and launched into their useful courses. We wish it may be at least weekly used for some years to come. The addresses were adapted to three epochs in the infant life of the Staines Society, which owes much of its rapid growth and flourishing state to this intelligent guide and eloquent advocate.

We regret that we have not room for large quotations, by which some of the liberal sentiments and powerful arguments in favour of an unsparing distribution of knowledge, might have all the circulation which may be in our power to give. We are compelled to confine ourselves to an earnest recommendation of the perusal and dissemination of all the three discourses; and the selection of the following passage:—

> "Terms of art have now become 'household words;' and the very pastimes of youth have changed. I see no reason why science and amusement should not be blended;—or why the boy should not be classing and labelling stones and fossils, employed by his father at his age as mere missiles of mischief. Whether this general instruction will ultimately make wiser and more useful men, remains to be seen. I think it will. I have ever lamented, that so many of the invaluable 'irrevocable years of youth are, must I say,

sacrificed to Greek and Latin, to the neglect,—often in contempt—of science, modern languages, and even of English literature. That some of the finest specimens of human genius are to be found in the authors of Greece and Rome, it would be absurd and plebeian to deny; and, perhaps, no liberal education could be complete, were the classics wholly omitted; but every mental attainment has its value and its use; let each have its merited rank and praise. Be the object of intellectual ambition what it may, there is a living link between all cultivated minds;—there is a golden band which ties together the sheaf of knowledge. I would not teach to the future tradesman the tragedies of Æschylus, or the dialogues of Plato;—nor would I mislead the future scholar into the by-gone fallacy, that there is neither merit nor fame but in scholastic prosody and syntax. Let us never cramp the ardent mind of youth by fetters, however venerable from custom they have become. Let us cherish talent, and prize knowledge, wherever we may find them. And this is applicable to all classes. If there be a general and a generous effort (and certainly there is) to instruct the children of the poor, why should we hesitate in carrying out such noble views—in perfecting what all allow to be a good beginning? If, by infant—Sunday—national—or other schools, we all agree—and all contribute—to prepare the mind of the rising generation for knowledge, surely we leave our task most imperfectly accomplished, if we do not supply the mental appetite, thus created, with safe and nourishing aliment. Are we to give the power to read, and then abandon the youthful craving mind to all the profane and ribald trash, that will be offered to its hunger? Inquisitive the popular mind will be—enlightened and moral it should be. Think for a moment of what you are doing—think of the co-existence of a reading populace, and a licentious unbridled press. Is it not religious—is it not wise—is it not mere common discretion—to provide, that the goodly seed, sown under your care, is duly tended in its after-growth, cleared from weeds, and fenced against dangers? Can there be a more ruinous error, than to confound the art of reading with education—the one a mere mechanical vehicle of knowledge—the other the business and the duty of our whole life?

"Our Institution is intended to meet and satisfy these very mental longings which instruction will engender. Hither, for the trifling sum of one shilling, the humblest amongst us may repair, and slake his thirst for knowledge*. Mere solitary unassisted study, is often more a task than a pleasure, and many are content to be ignorant, from the mere want of sympathy and encouragement. At a public lecture it is otherwise;—the very locality seems sacred to the cause of science and of letters, the companionship of the pursuit, the mere meeting together of those we love and value, cheers and prepares the mind for instruction. Many subjects are explained which books are unable sufficiently to illustrate;—access is afforded to the lecturer to remove difficulties, perpetually occurring in scientific treatises;—nay, the after conversation, which often ensues among the hearers of a lecture, clears away many a doubt, and leaves behind it many a kind sentiment, many a wise and gentle lesson. None but first-rate minds can hope to master the higher branches of science, unaided by experimental illustrations.

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* This is so precisely a portrait of the Adelaide Street Gallery and its objects, that we feel we may be liable to the suspicion of having selected the passage on this account, but it was, however, not so.

MISCELLANEOUS INTELLIGENCE.

STATUTE-LAW OF 1835.

WEIGHTS AND MEASURES. LETTERS PATENT.

IN examining the statutes which were enacted by the Imperial Parliament, in 1835, we find but two which come within our scope to report: one on Weights and Measures and one on Letters Patent. The reforms produced by either appear extremely meagre, when a comprehensive view is taken of what is desirable, and even of what is easily attainable, on these two important objects of legislative interference; but still the alterations are improvements, and though the moves are little ones, they are in advance. We propose never to lose sight of these widely influential questions, particularly that of improvements in the Patent law, convinced, from an inquiry into the effects of the Patent Regulations in other countries, that the inventors of our own are crushed by the weight of bad, unnecessary, and expensive law.

Of the act relating to Weights and Measures we present a copious and careful abstract. The other we give entire; it is short (unfortunately so) and a large portion of it consists in instructions for proceeding to obtain certain objects.

WEIGHTS AND MEASURES.

Act 5 & 6 Will. IV. c. 63. Royal Assent, 9 Sept. 1835.

An Act to repeal an Act, 4 & 5 Will. IV., relating to Weights and Measures, and to make other provisions instead thereof.

1. Act 4 & 5 Will. IV. repealed.

2. Nothing herein to interfere with anything done under the repealed act.

3. Provisions of 5 Geo. IV. c. 74, and 6 Geo. IV. c. 12, repealed; namely, those which require that Weights and Measures should be of the same form as the standards, and which allow the use of weights and measures not in conformity to these acts; and which allow the sale of goods, &c., by such weights and measures, and which allow the use of the heaped measure.

4. Weights and measures stamped at the Exchequer to be legal, though of dissimilar form to the standard.

5. Defective and mended copies of the standard must be re-verified.

6. The Winchester bushel, the Scotch ell, and all local and customary measures henceforth abolished. Penalty of using such, not exceeding 40*s*. Articles may be sold in vessels not representing any amount of imperial measure, nor being of any local measure heretofore in use.

7. Heaped measure henceforth abolished, penalty not exceeding 40*s*.

8. Articles formerly heaped to be sold by measures filled in all parts as nearly to the level of the brim as the size and shape of such articles will permit. These articles may be sold by weight also.

9. Coals, slack, culm, and cannel of every description, to be sold by weight after January 1st, 1836. Penalty, not exceeding 40*s*.

10. All articles sold by weight shall be sold by avoirdupois weight, except gold, silver, platina, diamonds, or other precious stones, which may be sold by troy weight: and drugs, which when sold by retail may be sold by apothecaries' weight.

11. Weight denominated a stone henceforth shall in all cases consist of 14lbs. avoirdupois. The cwt. shall be eight of such stones. The ton twenty such cwt.

12. Weights of a pound upwards shall have their number of pounds expressed upon them in legible characters. Measures of capacity shall also have their contents marked in the same manner on their outsides.

13. Weights of lead and of pewter not to be used after January 1st, 1836; but these materials may be used to fill up and adjust weights of brass, copper, and iron. When used for filling up, the weights must be stamped with the word "cased."

14, 15. Rents, tolls, &c., payable on grain, &c., according to former weights and measures, to have their value in the new ascertained by Jury, &c., in England, Scotland, and Ireland.

16. The fiar prices of grain in Scotland shall be struck by the imperial quarter only. Penalty, not exceeding 5*l.*

17. 19. 21. Copies of the imperial standards and stamps to be provided, and inspectors to be appointed in every county, by sessions in England, by justice-meetings in Scotland, and by grand-juries in Ireland.

18. Act not to operate in Orkney and Zetland until May 1st, 1836.

20. Judge may order copies of standards if Irish grand-juries do not.

21. See 17 as to the stamps. Fees for stamping to be paid as per schedule annexed. Penalty for using illegal weights and measures, not exceeding 5*l.*, and all sales, &c., by them null and void. No weight above 56lbs. need be inspected or stamped; nor wooden and wicker measures used in the sale of lime or similar articles; nor glass nor earthenware cups and jugs, though represented as containing some amount of imperial measure, but the person selling by such wooden, glass, and earthenware vessels may be required to produce a legal measure and compare them. Penalty for refusing to produce such measure, and for deficiency in the vessels measured, not exceeding 5*l.*

22. Copies of standards to be paid for at the expense of counties.

23. Inspector must not be a weight-maker or seller; must give security in the sum of 200*l.*

24. Inspector to attend at market-towns and examine, compare, and stamp, all weights and measures brought; and upon all weights above a quarter of a pound shall stamp his district number; shall keep a register of comparisons, and give certificate of such when registered; shall pay over all fees he receives to county treasurer, &c.

25. Berwick-on-Tweed, and other places authorized by charter, act of parliament, &c., may appoint inspectors, who shall have same powers and duties as the others.

26. Weigh-masters in Ireland shall be furnished with copies of standards.

27. Weights and measures once stamped need not be re-stamped when used in another place than that at which they were originally stamped.

28. Justices and authorized inspectors may enter any shop, &c., where goods are kept for sale or weighed for conveyance, and examine all weights, measures, and weighing machines: may seize the unjust ones, and fine the possessor. Penalty, not exceeding 5*l.* Persons refusing to produce their weights, measures, and weighing machines. Penalty, same.

29. Inspectors to be fined for neglect or misconduct. Penalty, not exceeding 5*l.*

30. Counterfeit stamps to be seized and broken up. Makers of such to be fined, not exceeding 50*l.* nor less than 10*l.* Sellers of such to be fined, not exceeding 10*l.* nor less than 2*l.*

31. Clerks of markets, &c., shall make their returns in legal imperial weights and measures. Penalty, not exceeding 10*s.*, for every copy of such returns which shall be otherwise filled up.

32. Appropriation of penalties. Not exceeding moiety to informer: the remainder to county treasurer, &c.

33. Suing of penalties. Form of conviction.

34. Receiving of such.

35. Persons aggrieved may appeal to quarter-sessions, &c.

36. Proceedings not to be quashed for want of form.

37, 38. Penalties, and appeal against, in Scotland.

39. Limitation of actions.

40. Plantiff wrongfully proceeded against, &c., shall not recover after tender of amends.

41. Act 4 Anne (I.) and 5 Geo. IV. c. 110. repealed so far as relates to weigh-masters.

42. Powers of ward inquests of London and Southwark reserved.

43. Same of Founders Company of London.

44. Same of Universities of Oxford and Cambridge.

45. Same of Leet-juries, &c.

46 and last. Act may be altered or repealed in present session.

Schedule of fees for examining, comparing, and stamping weights and measures.

Note. This Act was not altered or repealed during the Session in which it was passed.

LETTERS PATENT.

Act 5 & 6 Will. IV. c. 83. Royal Assent, 10 Sept., 1835.

An Act to amend the Law touching Letters Patent for Inventions.

Whereas it is expedient to make certain additions to, and alterations in, the present law touching letters patent for inventions, as well for the better protecting of patentees in the rights intended to be secured by such letters patent, as for the more ample benefit of the public from the same: Be it enacted by the king's most excellent majesty, by and with the advice and consent of the lords spiritual and temporal, and commons, in this present parliament assembled, and by the authority of the same, That any person who, as grantee, assignee, or otherwise, hath obtained or who shall hereafter obtain letters patent, for the sole making, exercising, vending, or using of any Invention, may, if he think fit, enter with the clerk of the patents of England, Scotland, or Ireland, respectively, as the case may be, having first obtained the leave of his majesty's attorney-general or solicitor-general in case of an English patent, of the lord-advocate or solicitor-general of Scotland in the case of a Scotch patent, or of his majesty's attorney-general or solicitor-general for Ireland in the case of an Irish Patent, certified by his fiat and signature, a disclaimer of any part of either the title of the invention or of the specification, stating the reason for such disclaimer, or may, with such leave as aforesaid, enter a memorandum of any alteration in the said title or specification, not being such disclaimer or such alteration as shall extend the exclusive right granted by the said letters patent; and such disclaimer or memorandum of alteration, being filed by the said clerk of the patents, and enrolled with the specification, shall be deemed and taken to be part of such letters patent or such specification in all courts whatever: provided always, that any person may enter a caveat, in like manner as caveats are now used to be entered, against such disclaimer or alteration; which caveat being so entered shall give the party entering the same a right to have notice of the application being heard by the attorney-general or solicitor-general or lord-advocate respectively: provided also, that no such disclaimer or alteration shall be receivable in evidence in any action or suit (save and except in any proceeding by *scire facias*) pending at the time when such disclaimer or alteration was enrolled, but in every such action or suit the original title and specification alone shall be given in evidence, and deemed and taken to be the title and specification of the invention for which the letters patent have been or shall have been granted: provided also, that it shall be lawful for the attorney-general or solicitor-general or lord-advocate, before granting such fiat, to require the party applying for the same to advertise his disclaimer or alteration in such manner as to such attorney-general or solicitor-general or lord-advocate shall seem right, and shall, if he so require such advertisement, certify in his fiat that the same has been duly made.

Any person having obtained Letters Patent for any Invention, may enter a disclaimer of any part of his specification, or a memorandum of any Alteration therein, which, when filed, to be deemed part of such specification.

Caveat may be entered as heretofore.

Disclaimer not to affect actions pending at the time.

Attorney General may require the party to advertise his disclaimer.

2. And be it enacted, That if, in any suit or action, it shall be proved or specially found, by the verdict of a jury, that any person who shall have obtained letters patent for any invention or supposed invention was not the first inventor thereof, or of some part thereof, by reason of some other person or persons having invented or used the same, or some part thereof, before the date of such letters patent, or if such patentee or his assigns shall discover that some other person had, unknown to such patentee, invented or used the same, or some part thereof, before the date of such letters patent, it shall and may be lawful for such patentee or his assigns to petition his majesty in council to confirm the said letters patent or to grant new letters patent, the matter of which petition shall be heard before the judicial committee of the privy-council; and such committee, upon examining the said matter, and being satisfied that such patentee believed himself to be the first and original inventor, and being satisfied that such invention or part thereof had not been publicly and generally used before the date of such first letters patent, may report to his majesty their opinion that the prayer of such petition ought to be complied with, whereupon his majesty may, if he think fit, grant such prayer; and the said letters patent shall be available in law and equity to give to such

Mode of proceeding where Patentee is proved not to be the real Inventor, though he believed himself to be so.

petitioner the sole right of using, making, and vending such invention as against all persons whatsoever, any law, usage, or custom to the contrary thereof notwithstanding: provided, that any person opposing such petition shall be entitled to be heard before the said judicial committee: provided also, that any person, party to any former suit or action touching such first letters patent, shall be entitled to have notice of such petition before presenting the same.

If, in any action or suit, a verdict or decree shall pass for the Patentee, the Judge may grant a certificate, which being given in evidence in any other suit, shall entitle the Patentee, upon a verdict in his favour, to receive treble costs.

3. And be it enacted, That if any action at law or any suit in equity for an account shall be brought in respect of any alleged infringement of such letters patent heretofore or hereafter granted, or any *scire facias* to repeal such letters patent, and if a verdict shall pass for the patentee or his assigns, or if a final decree or decretal order shall be made for him or them, upon the merits of the suit, it shall be lawful for the judge before whom such action shall be tried to certify on the record, or the judge who shall make such decree or order to give a certificate under his hand, that the validity of the patent came in question before him, which record or certificate being given in evidence in any other suit or action whatever touching such patent, if a verdict shall pass, or decree or decretal order be made, in favour of such patentee or his assigns, he or they shall receive treble costs in such suit or action, to be taxed at three times the taxed costs, unless the judge making such second or other decree or order, or trying such second or other action, shall certify that he ought not to have such treble costs.

Mode of proceeding in case of application for the prolongation of the term of a patent.

4. And be it further enacted, That if any person who now hath or shall hereafter obtain any letters patent as aforesaid shall advertise in the London Gazette three times, and in three London papers, and three times in some country paper published in the town where or near to which he carried on any manufacture of any thing made according to his specification, or near to or in which he resides in case he carried on no such manufacture, or published in the county where he carries on such manufacture, or where he lives in case there shall not be any paper published in such town, that he intends to apply to his majesty in council for a prolongation of his term of sole using and vending his invention, and shall petition his majesty in council to that effect, it shall be lawful for any person to enter a caveat at the council-office; and if his majesty shall refer the consideration of such petition to the judicial committee of the privy-council, and notice shall first be by him given to any person or persons who shall have entered such caveats, the petitioner shall be heard by his counsel and witnesses to prove his case, and the persons entering caveats shall likewise be heard by their counsel and witnesses; whereupon, and upon hearing and inquiring of the whole matter, the judicial committee may report to his majesty that a further extension of the term in the said letters patent should be granted, not exceeding seven years; and his majesty is hereby authorized and empowered, if he shall think fit, to grant new letters patent for the said invention for a term not exceeding seven years after the expiration of the first term, any law, custom, or usage to the contrary in anywise notwithstanding: Provided that no such extension shall be granted if the application by petition shall not be made and prosecuted with effect before the expiration of the term originally granted in such letters patent.

In case of action, &c., notice of objections to be given.

5. And be it enacted, That in any action brought against any person for infringing any letters patent, the defendant on pleading thereto shall give to the plaintiff, and in any *scire facias* to repeal such letters patent the plaintiff shall file with his declaration, a notice of any objections on which he means to rely at the trial of such action, and no objection shall be allowed to be made in behalf of such defendant or plaintiff respectively at such trial unless he prove the objections stated in such notice: provided always, that it shall and may be lawful for any judge at chambers, on summons served by such defendant or plaintiff on such plaintiff or defendant respectively to show cause why he should not be allowed to offer other objections whereof notice shall not have been given as aforesaid, to give leave to offer such objections, on such terms as to such judge shall seem fit.

As to costs in actions for infringing Letters Patent.

6. And be it enacted, That in any action brought for infringing the right granted by any letters patent, in taxing the costs thereof regard shall be had to the part of such case which has been proved at the trial,

which shall be certified by the judge before whom the same shall be had, and the costs of each part of the case shall be given according as either party has succeeded or failed therein, regard being had to the notice of objections, as well as the counts in the declaration, and without regard to the general result of the trial.

Penalty for using, unauthorized, the name of a Patentee, &c

7. And be it enacted, That if any person shall write, paint, or print, or mould, cast, or carve, or engrave or stamp, upon anything made, used, or sold by him, for the sole making or selling of which he hath not or shall not have obtained letters patent, the name or any imitation of the name of any other person who hath or shall have obtained letters patent for the sole making and vending of such thing, without leave in writing of such patentee or his assigns, or if any person shall upon such thing, not having been purchased from the patentee or some person who purchased it from or under such patentee, or not having had the license or consent in writing of such patentee or his assigns, write, paint, print, mould, cast, carve, engrave, stamp, or otherwise mark the word "patent," the words "letters patent," or the words "by the king's patent," or any words of the like kind, meaning, or import, with a view of imitating or counterfeiting the stamp, mark, or other device of the patentee, or shall in any other manner imitate or counterfeit the stamp or mark or other device of the patentee, he shall for every such offence be liable to a penalty of 50*l.*, to be recovered by action of debt, bill, plaint, process, or information in any of his majesty's Courts of Record at Westminster or in Ireland, or in the Court of Session in Scotland, one half to his majesty, his heirs and successors, and the other to any person who shall sue for the same: provided always, that nothing herein contained shall be construed to extend to subject any person to any penalty in respect of stamping or in any way marking the word "patent" upon anything made, for the sole making or vending of which a patent before obtained shall have expired.

Figure of the Earth's Surface in France.

M. Puissant, on the 11th Jan. read a paper before the Academy of Sciences, Paris, entitled, *Observations on the Comparison of Astronomical and Geodæsical Measurements in France.* M. Puissant commenced by observing, that in a memoir read before the Academy in 1833, it was proposed, that some conclusions as to the figure of the earth should be drawn from the comparison of the astronomical and geodæsical measurements which had served as bases for the new geographical Map of France. For this purpose he had made use of some differential formulæ to give the correction which must be made for the earth's ellipticity, in calculating the latitudes, longitudes, and azimuths, in order that these terrestrial elements might perfectly agree with the corresponding celestial determinations. These formulæ having simplified the means of successively combining the position of the Royal Observatory of Paris, with each of the astronomical stations with which it is connected by various chains of triangles, he found that no ellipsoid of revolution would pass exactly through or combine the whole of these stations, or, in other words, that the figure of the earth is very irregular in France.

The above paper was an extract from the second volume of the *New Geometrical Description of the Kingdom*, which M. Puissant is now preparing for the press. The object of this work is the elucidation of the above fact, by a comparison of the degrees of two meridians, those of Dijon and of Paris, the lengths of which have been determined by the trigonometrical operations of the geographical engineers of France. This comparison shows that the surface of France, at least that which has been surveyed, is formed by two very distinct curvatures; the one lying easterly, being a flattened spheroid, the other westerly, taking toward the south the form of an elongated one; and that in the same latitude, the lengths of the degrees of the meridians are very unequal. This inequality is, no doubt, the effect of a disturbing cause which variously affects the plumb-line.

M. Puissant added, in concluding, that the flattening of the earth estimated at $\frac{1}{305}$, agreeably to the measurements in France and in Peru, is more correctly, and conformably to the theory of the lunar inequalities, expressed by $\frac{1}{305}$, when the length of the meridional arc, comprised between Dunkirk and Montjouy, is corrected for the discordance of the bases of Melun and Perpignan, a discordance which has been recently detected by a better choice of triangles in that part of this arc which lies between the parallel of Forêt-Sainte-Croix, and that of Bourges.

Academy of Sciences, Paris.—Appointments.

PRESIDENT for 1836, M. Ch. Dupin.—Vice-president, M. Magendie; *vice* M. Ch. Dupin.—Member, (Section of Mineralogy and Geology,) M. Elie de Beaumont; *vice* M. Lelièvre, deceased.

Aspect of Halley's Comet.

Herr BESSEL was able to observe Halley's Comet for a period of nine hours, in the night of the 12th of last October, and remarked a luminous cone, which proceeded from the Comet in the direction of the radius vector, and vibrated in the plane of the Comet's orbit. Herr Bessel is occupied on a work in which the laws of this remarkable motion will be developed.

South colder than the North.

THE attention of meteorologists is requested to the fact, that in the two last months of 1835, the depression of the thermometer was greater, and commenced sooner in the south, than in the north, of France. And also, that in the Puy-de-Dôme, a department a little south of the centre of that country, it was not the north winds, but violent ones from the west and south, which produced the greatest cold.

Electro-chemical Decomposition unaccompanied by Evolution of Heat.

M. BECQUEREL has observed an electrical current which chemically decomposes bodies, but in which the property of heating them is absent. He thus describes the manner of producing it. When an electrical current from a voltaic apparatus passes through a saline solution, or a metallic wire sufficiently fine, the energy of the chemical and calorific effects depends upon the number and dimensions of the pairs of plates. The chemical effects are proportionate to the number of the pairs, and the calorific effects to their surface. Intensity is necessary to the first, and quantity to the second. There exists, also, such a relation between these two classes of phenomena, that the same current can produce them either simultaneously or separately, although in a very different degree. By the apparatus of M. Becquerel, described in our last number, this fact may be exhibited.

M. Becquerel commenced by satisfying himself from an enlargement of the apparatus, and the use of slips of platina having from a square half-inch of surface up to nearly 32 square inches, that a quantity of oxygen would be obtained, nearly in proportion to the surfaces. He succeeded in obtaining about 4½ cube inches of gas in twenty-four hours. There could be no doubt, owing to the great enlargement of the dimensions of the apparatus, that there had been an increase in the quantity of electricity disengaged during the reaction of the nitric acid on the potash: the diameter of the glass cylinder was 3 $\frac{1}{8}$ inches. The apparatus being arranged as before described, if the metallic circuit be interrupted in any point whatever, and the two free ends of the platina wire are dipped into two small cups of mercury, and the communication re-established by connecting these cups with a platina wire of $\frac{1}{2000}$ of an inch diameter, chemical decomposition will go on in the apparatus, without any sensible change. If then an ordinary multiplicator is introduced into the circuit to measure the intensity of the current, it will be found that this intensity suffers no alteration, whatever may be the diameter of the wire connecting the cups. Therefore a current producing such an abundance of gas passes with the same facility through a wire of extreme tenuity, as through one of a $\frac{1}{15}$ inch or more in diameter. This is not all: if the microscopical wire, through which is passing a large quantity of electricity, is placed opposite the orifice of a thermo-electrical apparatus (electrical pile) adapted to indicate very minute degrees of temperature, it will be found that the temperature of the microscopical wire does not vary at the moment of closing the circuit. Now this same wire will become red-hot, if it be placed in a position to establish the communication between the two elements even of the smallest voltaic apparatus possible, such as, for example, that which was constructed by Dr. Wollaston with a thimble.

M. Biot has made the following observations upon the above experiment of M. Becquerel. "It appears to me that this phenomenon should be attributed not to a modification of the electric principle, in which it is considered as deprived of its property of exciting heat, while it preserves its power of decomposing chemical combinations, but rather to a mode by which it is disengaged and transported by intermissions which are more or less rapid. Let us imagine that a certain definite quantity of electricity E is disengaged in a certain time T by two substances placed in contact as they are in the apparatus of M. Becquerel; let us divide the time into a number t of instants, during each of which a proportional element e of the total E is disengaged. Each of these small charges, passing through the conducting wire in a time which will not be sensible compared to T, will communicate to it the chemical and calorific properties which are due to its quantity and to the velocity of its passage. Now, in proportion as the intervals of the time t are short, so will the elementary charges e be small; and if they become sufficiently so as not individually to heat the wire in their passage, the total charge E will flow off without producing a sensible elevation of temperature in the wire. But if other substances are placed in contact in the same manner, or if the same are placed differently, it is possible that the intermissions may become more separated, this will make, in a certain proportion, each minute charge e greater for the same total expenditure E in the same time, and then it may be so done, as that in passing through the wire, they may sensibly elevate its temperature; whereas the same quantity E discharged in another manner, would not produce this effect."

Elephants, Hail, &c., in Abyssinia.

In Abyssinia, according to Herr Ruppell, elephants and monkeys do not fear to cross plains, some of which have an elevation of 8300 feet, and on which the temperature must be exceedingly low. In the same country it hails frequently, but never during storms. This fact renders the explanation of the formation of hail still more difficult, it having been supposed, up to the present time, that electricity played an important part in the process.

Application of Optics to Chemistry.

M. BIOT is occasionally developing to the Academy of Sciences his mathematical and experimental method of detecting mixtures and combinations, both definite and indefinite, which act upon polarized light, followed by its application to compounds of tartaric acid with water, alcohol, and pyroligneous acid.

Effect of the Green Colours of Porcelain on Blood.

Dr. NEWBIGGING, having been induced to observe certain appearances in coagulated blood after it had been for some time in contact with green-coloured surfaces, preserved blood in a china-saucer, the interior of which was glazed. He remarked, on the under-surface of the clot, brilliant spots, which exactly corresponded with the green-coloured parts of the saucer. All the other parts of the clot had the usual dull tint. Wishing to ascertain if these spots were owing to the action of the green colour, he repeated the experiment with different kinds of porcelain, glazed with various colours; the same result was uniformly obtained. The contrast which existed between the brilliant red parts of the clot and those which were dull and brown, continued for about ten minutes after exposure to the air.

Temperature of the Antilles.

THE cold weather in the Antilles began in the month of October in the last year, and gradually increased, until, at the end of November, it had become very severe and unusual. It was accompanied with violent squalls of rain. In reading accounts of this kind, it is very important to ascertain the precise meaning of such comparative terms, as cold, warm, &c. In the above case, most persons in this country will probably find their notions of the remarkable cold in question a little modified, when they learn that the thermometer throughout the whole region is, at the level of the sea, never below 72½ F.!

National Coast-Survey of the United States.

So far back as 1807, Mr. F. H. Hassler, a Swiss engineer, who had emigrated to America in consequence of the occupation of his country by the French, was employed by the government of the United States, to draw up a plan for the survey of the coasts of that country. The selection of this gentleman was justified by the experience and reputation he had acquired in a survey of his native Alps, and his plan was eventually approved, and ordered to be carried into execution. As at that time there were no instruments in the States by which such a survey could be made, Mr. Hassler was sent to England in 1811, with a credit of about 3000*l.*, to purchase instruments, &c., and to direct and superintend the construction of such others as he conceived might be necessary to be made expressly for the purpose in view. In London he formed an acquaintance with the late Mr. Troughton, and an order for some astronomical and surveying instruments was executed by that gentleman, under the eye of Mr. Hassler, and sent to the United States in 1812. These instruments Mr. Hassler describes as "the best collection that ever left England." The progress of the survey was, however, very slow; scarcely anything was attempted until 1816; and it closed entirely in 1817 or 1818, with the measurement of a base-line in New Jersey. For the next thirteen or fourteen years, the coast-survey was almost entirely neglected. In 1832, twenty-six years from the time it had been first proposed to him, we find this important survey placed again under the care of Mr. Hassler; and from that period, until the end of the past year, Mr. Hassler has been directing the operations on the eastern coast, principally on the shores of New York and New Jersey.

One of the first acts of Mr. Hassler, on his re-appointment to this surveyorship, was to order a theodolite of unusual power and dimension, for the purpose of carrying on the principal triangulation. This he confided to the care of Messrs. Troughton and Simms of London. The instrument has been recently finished by the surviving partner of that firm, Mr. Simms, and is now on its actual voyage to the United States. It was constructed agreeably to drawings sent to this country by Mr. Hassler; and in addition to every improvement that has been lately introduced into instruments of this kind, there are several novelties about it, which were suggested by Mr. Hassler. The following is a slight description of this valuable instrument.

The horizontal circle is thirty inches in diameter, and is divided *originally*, that is to say, it has not been divided by the engine, or by any other dividing instrument, but according to the method described by the inventor, Mr. Troughton, in the *Philos. Trans.* for 1809. The spaces upon the circle are subdivided into single seconds by micrometers. There are two telescopes of forty-five inches focal length; one of these is mounted like that of a transit-instrument. It can be employed as a watch-telescope, to detect any accidental motion of the horizontal circle, during the revolution of the superior parts; and also, when placed in proper supports above the horizontal circle, it may be used in the case when the horizontal angle, subtended by two remote objects, is the only result required. The second telescope has upon its axis a twenty-four inch double repeating circle, and is intended to be employed in such observations as have been, in surveys of this nature, hitherto made with the zenith-sector, or other astronomical instrument. This repeating circle is, we believe, one of the principal additions suggested by Mr. Hassler.

The apathy with which the American government and nation have regarded a survey so important to the maritime interests of the country is inconceivable. Thirty years have passed away since its commencement, and scarcely a fraction of the survey has been completed! We regret to add, after a perusal of the published documents relating to the operations during the last year, that we have little hope that they will, in the ensuing one, be carried on in the spirit, and on a scale, which the welfare of the shipping interests of a great nation, and the extension of accurate geographical knowledge on the Western hemisphere, would demand. While officers of the English East India Company are running along an unprecedented arc of a meridian in Hindoostan, the government engineers of the United States of America are scarcely allowed the means of covering Long Island with their triangulation.

Voluntary Instruction of the People.

One of the most pleasing features of the present state of things, is the interest which the higher and well-educated classes in many places are taking in the social improvement of those less favoured by fortune or circumstances. In Edinburgh, lectures are delivered nightly by gentlemen to thousands of people, on subjects of Physical and Moral Science. In one place, which contains an audience of two thousand persons, lectures, the admission to which is only a single penny, are delivered to the working classes, on Moral and Economical Science, or, in other words, on topics calculated to improve their mental faculties and condition in life. An analysis of these lectures is given in the *Edinburgh Chronicle* newspaper, weekly.

Coach-springs.

THESE indispensable requisites to all carriages are daily becoming more important. We are convinced that a proper investigation into their effects, their construction, and their position, would be a public benefit. The subject was lately brought under the attention of the French Academy of Sciences by M. Fusz, who presented some springs invented by himself. MM. Poncelet and Navier reported on their merits, and requested the Academy to express their approbation of the principle adopted by M. Fusz, but added that experience alone could decide upon the intrinsic and comparative value of the invention. We regret that the description given in the report is so general as to fail in conveying a definite idea of the principle recommended.

Road-indicator.

"IN noticing Sir Henry Parnell's paper on the Construction of Coaches, in your last number, you allude to the instrument which I invented some years ago, for the purpose of ascertaining the state of the surface of roads, and the power required to draw carriages over them, and state that whilst other countries were taking advantage of the information which this instrument would afford in ascertaining the proper construction of carriages, and the roads most suitable for draught, England alone, where every facility and accommodation in travelling was of so much national importance, seemed inattentive to the subject. On this point you were misinformed, and I am happy in having it in my power to state that I have been for several months employed in arranging and constructing a new instrument for the same object, by order of the Commissioners of his Majesty's Woods and Forests. There will be some novelties and additions in this, the fruit of a longer experience. The spring will act in a different way, its vibrations will not be checked by a piston passing through a fluid, and they will be all registered; the total amount of the impulses produced by the action of the horses' shoulders on the collar will be ascertained with great exactness; and the instrument will not only show by an index, the amount of power exerted on any part of the road, but will also register the pulls, without the assistance of an observer; and at the end of the journey the total amount of power required to draw the carriage over any road will be exhibited, as well as the amount of power at any particular part of the distance traversed. It will also mark the acclivities or declivities on the road, so as to give an accurate section of the road over which it travels. I shall be happy to transmit the designs as they are now executing, if you should think they deserve a place in a future number."—*Extract from a Letter of John Macneill, Esq., to the Editor.*

Stability of the Menai Suspension Bridge.

THE gale on the 23rd ult., in the Menai Strait, was probably the most violent that has happened since the Suspension Bridge was thrown across it. An intelligent eye-witness of the effect of the wind, has enabled us to state, that the wind, which was from the S.W.*, seemed to descend upon the bridge; and though it produced no lateral motion, it excited an undulation in the long line which is suspended between the supporting pyramids, to such an extraordinary degree, that the wave ran from end to end of the roadway, and measured vertically not less than sixteen feet, that is to say, that its crest was in one part

* The direction of the central line of the bridge is nearly N.W. and S.E., so that the wind acted nearly at right angles upon the Carnarvon side.

elevated eight feet above an horizontal line, and its hollow depressed, at another part, in the same instant of time, eight feet below it. The highest and lowest points of the wave, occurred at about half-way between the pyramids and the centre of the span. The undulation was steady and uniform, but the swell across the roadway was not so, and this threw it out of level, one side being the highest at one moment and the other side at another. This irregularity disturbed some of the planking, and broke a few of the vertical suspender-rods, and some of the small braces which connect the suspending cables; but the saddles on the top of the pyramids, which connect the centre suspending-cables with those which run to either shore, were not in the least degree disturbed. As the wind lulled, the undulation subsided, and carriages, &c., immediately crossed as usual. The whole cost of the damage incurred during this severe gale, which lasted twelve hours, will not amount to more than 20*s.* or 30*s.*! To estimate, in some degree, the power of the master-mind that designed and directed the execution of a work, which could thus endure harmless such an outpouring of this destructive agent; it should be recollected that the height of these obelisks above the sea, low-water, is 173 feet, that they are 552 feet asunder, and that the weight that swings between them is, at least, 650 tons, suspended at a height of 121 feet above low water. Its stability after a storm, which produced such remarkable effects, would have been matter of high exultation to its engineer if he had been living. We think that on the monument which is in progress to be raised to his memory, or in the medals which he left as premiums by his will for the promotion of science, now in the hands of an eminent artist, some memorandum of this triumphant struggle might be recorded. We cannot forbear saying more, and asking,—Why should not engraving be employed monumentally! In the case of a similar work of genius, we think that the happy representation of the Eddystone Light-house, after a gale from the S.W., has contributed more to the extension of Smeaton's fame, and the preservation of his memory, than adding twenty blocks to the great marble quarry which already exists in Westminster Abbey. We should be deeply interested to see the genius of English sculpture reject the worn-out common-places, and connect the name of Telford with the Menai Bridge, in a composition worthy of their high reputation. Is Roubiliac to be for ever a solitary instance of the pre-eminence of common-sense? And is the statue of Newton and his prism to be for ever unique in conception?

Practical Improvement in Light-House Illumination.

On the evening of the 1st of October last, a new light on the dioptric* principle of Fresnel, was exhibited on the island of Inchkeith, in the Firth of Forth, in the place of the reflecting light which had been used there, and which was discontinued on the 30th of September. The new light is distinguished, like the old one, from others in the neighbourhood, by flashes, occurring once in a minute, but it is very far superior in brilliancy and magnitude. Its power, compared to its predecessor, is as 2½ to 1. The cost of its maintenance is however greater, being as 17 to 7.

The light-house on the Isle of May, in the same neighbourhood, is in the course of an improvement of the same kind.

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* In this principle, the light is *transmitted through* media as lenses, and not *reflected from* surfaces, as the British lights usually are.

NEW PATENTS.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.

JANUARY.

1. James Champion, of Salford, Lancash. machine-maker; for certain improvements in machinery for spinning, twisting, and doubling cotton and other fibrous substances. Jan. 6.—July 6.
2. John Ramsbottom, of Todmorden, Lancash., mechanist; for certain improvements in machinery for roving, spinning, and doubling cotton and other fibrous substances. Jan. 6.—July 6.
3. William Harter, of Manchester, silk manufacturer; for certain improvements in machinery for winding, cleaning, drawing, and doubling hard and soft silk. Jan. 8.—July 8.
4. Francis Brewin, Kent-road, Surrey, tanner; for certain new and improved processes of tanning. Jan. 11.—July 11.
5. J. Tilton Slade, of Fitzroy-square, Middx., gent., for certain new and improved machinery for raising earth and for other useful purposes. Jan. 11.—July 11.
6. John Ward Higham, of Tavistock-st., Middx., for an improved tablet for sharpening of razors, penknives, surgical instruments, chisels, plane-irons, and other steel instruments which are capable of being sharpened by what are commonly called hones, Turkey-stones, or Welch-stones. Jan. 11.—Mar. 11.
7. John Burns Smith, of Salford, Lancash., cotton-spinner, and John Smith, of Halifax, York, dyer; for certain methods of tentering, stretching, or keeping out cloth to its width, made either of cotton, silk, wool, or any other fibrous substances, by machinery. Jan. 14.—July 14.
8. Moses Poole, of Lincoln's-inn, Middx., gent., for improvements in Jacquard looms: being a communication from a foreigner residing abroad Jan. 19.—July 19.
9. Charles Brandt, of Upper Belgrave-place, Middx., mechanist; for certain improvements in heating, evaporating, and cooling fluids. Jan. 19.—July 19.
10. Francis Moll, of Grove-lane-terrace, Camberwell, Surrey, esq., for improvements in preserving certain vegetable substances from decay. Jan. 19.—July 19.
11. Charles Harsleben, of Bold-street, *Liverpool, Lancash.*, esq.; for certain improvements in the machinery and arrangements for the use of propelling vessels and other floating bodies, as also carriages and other vehicles on railroads, as well as on common roads, part of which machinery is also applicable to other purposes. Jan. 19.—July 19.
12. Robert Bowie, of Bishopsgate-street within, London, surgeon; for certain improvements in distillation and decoction, which improvements are more or less applicable to the heating of fluids of all descriptions; as also to the purification of oleaginous bodies, both animal and vegetable. Jan. 21.—July 21.
13. John Ferrabee, of the Thrup, Stroud, Gloucestersh., engineer, and Richard Clyburn, of the same place, engineer; for certain improvements in power-looms. Jan. 21.—July 21.
14. William Burch, Borough-road, Surrey, calico and silk printer; for certain improvements in machinery for printing silk and cotton net, or lace. Jan. 23.—July 23.
15. Julius Jeffreys, of Osnaburgh-street, Regent's Park, Middx., esq.; for improvements in curing or relieving disorders of the lungs. Jan. 23.—July 23.
16. Henry Booth, of Liverpool, Lancash., gent.; for improvements applicable to locomotive steam-engines and railway-carriages. Jan. 23.—Mar. 23.
17. Henry Pickworth, the younger, Sipson, Middx., gent.; for certain improvements in machinery for propelling vessels and other floating bodies, moved by steam or other power. Jan. 26.—July 26.
18. John Filmore Kingston, of Islington, Devonsh.; for a new rotary engine. Jan. 28.—July 28.
19. William Boulnois, the younger, of Gower-street, London; for an improved combination or arrangement of springs for carriages. Jan. 30.—July 30.

FEBRUARY.

20. Stephen Reed, Newcastle-on-Tyne, gent.; for two hooks and an improved bow for corves, baskets, buckets, &c., which are conveyed, either loaded or empty, from one level to another, by being let down or drawn up, in mines, pits, and in other works; and in ships and other vessels, where cranes, &c., are now used. Feb. 1.—April 1.

21. John Baring, of Bishopsgate-street, London, merchant; for certain improvements in machinery for dressing wool; being a communication from a foreigner residing abroad. Feb. 3.—Aug. 3.

22. Frederick Edward Harvey, mechanical draughtsman, and Jeremiah Brown, roll-turner, of Tipton, Staffordsh.; for certain improvements in the process and machinery for making metallic tubes, and for forging or rolling metal. Feb. 3.—Aug. 3.

23. Edmund Ashworth, cotton-spinner, and James Greenough, overlooker, of Egerton, Lancash.; for certain improvements in machinery for preparing and spinning cotton, silk, wool, &c. Feb. 5.—Aug. 5.

24. Henry Adcock, of Stamford-street, Blackfriars, Surrey, civil-engineer; for certain improvements in the loading and unloading of ships, &c., especially applicable to those called colliers, and which discharge in the Pool. Feb. 5.—Aug. 5.

25. Alexander Massie and Robert Morton, of Wapping, Middx., engineers, and William Ranwell, coal-merchant, and Ebenezer Ranwell, miller, both of Woolwich, Kent; for certain improvements in the construction of paddle-wheels for propelling vessels: which are also applicable to water-wheels for Mills. Feb. 9.—Aug. 9.

26. Frederick Herbert Maberly, of Bourne, Cambridgesh., clerk; for improved machinery for cleaning roads or streets. Feb. 10.—Aug. 10.

27. Samuel Fenton, of Fishguard, Pembrokesh., S. W., clerk; for an improvement in the construction of locks and latches for doors, gates, &c. Feb. 10.—Aug. 10.

28. John Howard Kyan, of Twickenham, Middx., esq.; for a new mode of preserving certain vegetable substances from decay,—to extend only to our colonies and plantations abroad. Feb. 11.—April 11.

29. Andrew Smith, of Princes-street, St. Martin's in the Fields, Middx., engineer; for certain improvements in engines for driving machinery, and for raising and lowering heavy bodies. Feb. 12.—Aug. 12.

30. Charles Shafhanth, of Sheffield, Yorksh., gent.; for an improved steam-generator. Feb. 16.—Aug. 16.

31. Joshua Procter Westhead, of Manchester, Lancash., small ware manufacturer; for an improved method of cutting India rubber, leather, hides, &c. Feb. 16.—Aug. 16.

32. Michael Hodge Simpson, of Ludgate-hill, London, merchant; for certain improvements in machinery for dressing hemp, flax, tow, &c., and also waste silk; being a communication from a foreigner residing abroad. Feb. 17.—Aug. 17.

33. Joseph Lidel, of Arundel-street, Panton-square, Middx., professor of music; for certain improvements in piano-fortes; being a communication from a foreigner residing abroad. Feb. 17.—Aug. 17.

34. William Bucknall, of Crutched-friars, London, cork-merchant; for improvements in machinery for propelling vessels, and for water-wheels. Feb. 17.—Aug. 17.

35. Frederick Chaplin, of Bishops' Stortford, Herts, tanner; for an improvement in tanning hides of certain descriptions. Feb. 18.—Aug. 18.

36. Henry Martinson Robinson, of the Minories, London, varnish manufacturer; for improvements in certain descriptions of lamps. Feb. 18.—Aug. 18.

37. John Barsham, of Stepney Causeway, Middx., oxalic acid manufacturer; for improvements in the manufacture of oxalic acids and salacetecella. Feb. 20.—Aug. 20.

38. François Peyre, junior, of St. Etienne, France, now residing at the White Hart Inn, Southwark, Surrey, dyer; for improvements in economizing fuel in ships' hearths, &c., and of obtaining distilled water from sea water, and which apply to generating steam; being a communication from a foreigner residing abroad. Feb. 23.—Aug. 23.

METEOROLOGICAL JOURNAL FOR JANUARY, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month	Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer		Solar Var.	Daily Temp.	Rad.	Clouds 0—10		Wind. 0—6		Direction of wind		Lunation.	WEATHER, &c.
					Min.	Max.								A.M.	P.M.		
Friday, 1	30·450	43°	30·525	43°	26°7	32°0	5·3	29°3	24°	10	10	3	3·2	E.	E.		Small driving snow and sleet; fine night. [at night.
Satur. 2	30·782	38	30·750	39	12·6	27·0	14·4	19·8	9	0	10	0	0·1	E.	S.S.W.	○	Intense frost; clear A. M.; cloudy P. M.; light snow
SUNDAY, 3	30·600	40	30·551	43	27·5	40·4	12·9	33·9	27	10	9	2	2	W.S.W.	W.S.W.		A perfect thaw; *scud* and *cirro-cum.*; Small rain.
Monday, 4	30·348	44	30·320	48	36·9	48·7	11·8	42·8	32	10	10	3	3	W. b S.	W.		Gale of wind with *scud*; high temperature.
Tuesday, 5	30·362	49	30·370	54	43·5	50·9	7·4	47·2	41	5	4	1	1	W.	W.		Mild; clouds light and broken.
Wednes. 6	30·352	54	30·302	54	43·0	47·5	4·5	45·2	43	10	10	0	0·1	S.	S.S.E.		Cloudy; drizzling rain.
Thurs. 7	30·100	54	30·059	53	32·1	39·0	6·9	35·5	29	10	10	1	1	S.E.	S.		Do. do.
Friday, 8	30·104	53	30·140	53	36.0	41·9	5·9	39·0	33	10	5	1	1	S.S.E.	S.E.		Do. evening misty, with *scud*.
Satur. 9	30·119	49	30·052	49	26·7	33·4	6·7	30·0	25	8	10	1	1	E.	E.		Frosty; misty; a little sleet at evening.
SUN. 10	29·768	45	29·521	45	29·1	32·0	2·9	30·5	29	9	10	2	3·1	E.	E.		Snow early A. M.; P. M. a heavy snow.
Mon. 11	29·412	45	29·425	46	29·5	36·5	7·0	33·0	29	7	10	0	1	Calm.	E.S.W.	☾	Snow 4 inches deep; heavy rain from 7 to 11 P. M.
Tues. 12	29·560	45	29·600	45	25·7	35·5	9·8	30.6	25	2	3	1	1	S.W.	S.W.		Sharp frost; fine.
Wed. 13	29·958	44	30·001	45	26·5	39·0	12·5	32.7	23	1	4	2	2	W.S.W.	S.W.		*Cirro-cum.*; fine.
Thurs. 14	30·050	48	29·952	49	37·5	48·5	11·0	43·0	34	7	8	3	4·5	W.S.W.	W.S.W.		Strong wind; *cirro-stratus*; *scud*; stormy night.
Friday, 15	29·601	49	29·630	50	46·0	47·0	1·0	46·5	40	7	4	3	3	W. b N.	W.		High wind; *cumuli*; air sharp and cold.
Satur. 16	30·235	48	30·340	48	30·4	35·1	4·7	32·7	27	0	1	1	1	N.W.	N.W.		Hoar-frost; cloudless, with haze.
SUN. 17	30·396	45	30·435	48	26·2	38·0	11·8	32·1	22	1	2	1	1	W.	W.		Do. strong ground-frost.
Mon. 18	30·377	45	30·189	47	28·6	44·6	16·0	36·6	24	9	9	2	3	W.S.W.	W.S.W.		Windy.
Tues. 19	30·375	46	30·450	47	32·0	37·6	5·6	34·8	28	2	5	1	1	N.E.	E.S.	●	Air frosty; *cirro-cum.* in flocks.
Wed. 20	30·400	46	30·315	48	25·2	42·0	16·8	33·6	22	10	10	2	2	S.W.	S.S.W.		Raw damp air; *cirro-cum.* in flocks; *scud*.
Thurs. 21	30·018	48	29·905	48	32·0	40·0	8·0	36·0	28	7	10	2	2	S. b E.	S.		*Cirro-cum.*; *scud*; hollow wind.
Friday, 22	29·700	49	29·736	49	38·1	49·1	11·0	43·6	35	10	9	2	3	S.S.W.	S.S.W.		*Cirro-stratus*; wind.
Satur. 23	29·592	52	29·750	54	43·2	53·6	10·4	48·4	41	6	2	2	4	W.S.W.	W.S.W.		Rain and wind A. M.
SUN. 24	30·182	54	30·165	54	42·9	48·2	5·3	45·5	39	2	4	3	1	W. b S.	W. b S.		Windy; *scud*; but very fine.
Mon. 25	30·475	53	30·495	55	38·0	45·0	7·0	41·5	35	10	4	0	1	S.W.	S.W.S.	☽	*Cirro-cum.*; *stratus*; open weather.
Tues. 26	30·381	52	30·290	52	34·1	45·4	11·3	39·8	30	8	9	1	2	S. b W.	S.S.W.		*Cirro-cum.*
Wed. 27	30·200	53	30·102	53	37·0	44·5	7·5	40·7	32	10	5	1	2	W.	S.W.		Rainy A.M.; afternoon fine; wind very high at night.
Thurs. 28	29·905	53	29·774	54	39·6	49·1	9·5	44·3	35	4	7	2	2·3	S.W.	SW. b W		Fair A. M.; a violent squall of hail and rain at mid-
Friday, 29	29·650	50	29·501	51	34·0	41·5	7·5	37·7	31½	5	10	3	1	W. b S.	S.W.		Air sharp; evening rain and snow. [night.
Satur. 30	29·292	48	29·425	50	30·8	43·3	12·5	37·1	29	9	2	2	3	W.	N.W.		Snow and sleet A. M.
SUN. 31	29·601	48	29·400	50	30·5	47·5	17·0	39·0	28	10	5	2	3	S. b W.	W.S.W.		Rain till 2 P. M.; clouds broken.
Mean	30·078	48	30·054	49	32·82	41·92	9·07										Mean Barom. 30° 066. Mean Ther. 41° 2.

Bar. Max. 30·782 on the 2d. | Mean height at 9 A. M. 30·078 } M. Press || Ther. Max. 53°6 on the 23d. } Mean Tem. || Lowest point of Rad. 9, on the 2d.

Bar. Min. 29·040 29th. | Mean do. at 3 P. M. 30·054 } 30·066 || Ther. Min. 12°6 2d. } 37°37. || Solar var. 9°07. Rain and snow fallen 2·03.

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RECENT RESEARCHES ON HEAT.

In endeavouring to give our readers slight sketches of the recent progress and present condition of different leading branches of physical science, we are of necessity led to take somewhat of a retrospective view, were it only for the purpose of making our descriptions generally intelligible. The explanation of the terms employed, is, generally speaking, best attained by looking back to the researches which gave birth to them. But this almost unavoidably carries our discussions to some length. In the present instance, we fear the subject itself may be imagined of too abstruse and dry a character, to render such length endurable. Yet we must bespeak our readers' patience in the outset; and only trust we may, in the course of our account, so far engage their interest in a very beautiful department of experimental inquiry, as to afford some apology for the details on which we must enter, or run the risk of being unintelligible. With this preliminary remark, then, we will advance to the subject of heat; a very small portion of whose effects have as yet received such an examination, as to lead to anything like well-established laws.

M. Melloni's Researches.

Though the "thermo-multiplier" of M. Melloni has now been for several years before the scientific world, it is not yet perhaps so generally known to experimenters as it deserves to be. We shall deem it, therefore, not inappropriate to the nature of this article, to state briefly its principle.

The essential part of it consists in a great number of pairs of small slips of antimony and bismuth soldered together, and combined in one case, so as to have their galvanic action excited by the application of heat. This thermo-electric effect is indicated and measured by its influence on a magnetic needle, placed below, and arranged as a galvanometer, by having many coils of wire passed round it, the wires communicating with the thermo-electric combination; the effect is increased in proportion to the number of pairs of plates. Thus the *galvanic* action on the *needle* is the measure of the amount of *heat* affecting the metallic combination; and the important and valuable part of the contrivance is, that degrees of heat, so small as to be quite insensible to the most delicate thermometers, are *multiplied*, as it were, by the multiplication of the number of pairs of metal plates, and thus produce a sensible effect on the galvanometer needle. The skill of artists has been exercised in reducing them to small dimensions. M. Gourjon of Paris has succeeded in bringing them into so small a compass, that the end of the case which is exposed to the heat, is not greater than the section of the bulb of an ordinary thermometer.

An instrument so far surpassing all formerly known in the sensibility of its indications, has, in the hands of its distinguished inventor, led to a series of results equally new and remarkable. A brief summary of some of the chief of them is as follows:

Radiant heat passes directly, in greater or less quantity, through certain kinds of solid and liquid bodies. This class of bodies does not precisely include those which are transparent, since some which are opaque, or very little transparent, are the most "diathermal[*]," that is, transparent, as it were, to *heat*. This term is one which M. Melloni has introduced, as descriptive of the characteristic in question, and which we shall continue to use.

He concludes, in general, that there exist different species of heating rays; and that all these different kinds are emitted simultaneously from luminous hot bodies, though in different proportions from different sources, certain of them are entirely wanting in non-luminous hot bodies.

Rock-salt, cut into plates, and successively exposed to the radiations from different sources, transmits in all cases the same proportion of heat. Plates of any other diathermal substance, under the same circumstances, transmit a less proportion of heat as the temperature of the source is less elevated; but the differences between one substance and another in this respect, diminish as the plate is of less thickness; whence it follows (according to M. Melloni) that the calorific rays from different sources are intercepted in a greater or less degree, not at the *surface*, or in virtue of an absorbing power which varies with the intensity, but in the *interior* of the plate, by *a peculiar absorptive force, which is analogous to that of coloured media for particular rays of light.*

M. Melloni advances several theoretical views in support of this analogy. He remarks in general that there is but one substance (*viz.* rock-salt) of all he has tried, which is transparent and uncoloured, and acts really in the same manner both on the rays of light and of heat. All others, though they allow all rays of light to pass indifferently, yet absorb certain rays of heat and transmit others. We thus recognise, by means of these bodies, a true distinction in heat corresponding to that of colour in light.

The colouring matter of transparent media always diminishes more or less their diathermal properties, but gives them no peculiar property of stopping by preference any particular species of heating rays. It operates upon the transmission of radiant heat, as *brown* colouring matter (a smoked glass for instance) does upon light; that is, has only a general diminishing power on the intensity. There seems to be, however, an exception in regard to certain glasses coloured with green and opaque black; but these two kinds of colouring matter only appear to act in modifying the quality of the diathermal property.

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[*] From the Greek, δια, *through*; and θερμος, *hot.*

Glass intercepts wholly certain species of heating rays, including all those which come from bodies below luminosity; hence, in this last case, no refraction by prisms or lenses has ever been effected for such rays of heat. With rock-salt, however, the case is different; and it is unquestionably the most singular and important of the facts elicited by Melloni, that simple heat is not merely transmitted through rock-salt, but absolutely *refracted* by it. He determined this both by a lens, and still more remarkably, by a prism of that substance.

With the prism interposed in the path of the rays coming from the source of heat, the effect was no longer transmitted in a straight line, but made to undergo a considerable deviation by the action of the prism. This was observed to take place in different degrees, according to the nature of the source of heat. The greatest deviation took place when the flame of a lamp was employed; the next with incandescent platinum; the next with copper, at 390° centig., and when a vessel of boiling water was substituted, the effect was found too feeble to allow of any comparison with the other cases.

However, when the rock-salt was cut in the form of a lens, the concentration of the rays, even from boiling water, was sufficient to give a decided proof of their being really brought to a focus.

Questions relating to the *transmission* of radiant heat through different media, are those which have formed the principal subject of M. Melloni's inquiries. But he has also, in one instance, directed his attention to the equally curious and important question, of the relation of the state of the *surfaces and colour* of bodies to heat. The instance referred to is an examination of the *combination* of the effect of a *screen* with that of *surfaces*, the very same, in fact, which constitutes the experiment first proposed and tried by Mr. Powell, and published in the *Phil. Trans.* for 1825. This fundamental experiment M. Melloni has repeated, and has completely verified it with his extremely accurate apparatus; a confirmation the more valuable, as some previous experimenters, since the date of the publication of the original investigation, seem to have overlooked it. It decisively proves that that portion of the heat from a flame, which passes through a glass screen, is *also* distinguished from the part which is intercepted, by the additional characteristic of affecting a *black and a white* surface in a *different ratio*. This is an inquiry eminently deserving to be followed up by the same method, and to be extended to a long series of different sorts of coating applied to the thermometer or thermo-multiplier.

POLARIZATION OF HEAT.—PROFESSOR FORBES.

M. Melloni failed in obtaining any detection of the effects due to the *polarization of heat*, which had been originally stated by M. Berard, though subsequent inquirers had been unable to discover any traces of it.

That zealous and highly-talented experimentalist, Professor Forbes of Edinburgh, here took up the subject. We believe we may say he was the first to introduce the use of Melloni's instrument into Great Britain. He has certainly employed it with signal success. Of such delicate inquiries as those respecting polarization, involving, in fact, complicated arrangements, which could hardly be made intelligible without lengthened details, it would not be possible to speak, in so rapid a sketch as the present, in a way to do them justice. But we must mention, however briefly and imperfectly, the valuable conclusions to which Professor Forbes's labours have led. In an elaborate and masterly paper in the *Edin. Trans.* (vol. xiii.), he has detailed these important researches. The analogies afforded by the polarization of light, led him to expect the most probable method of succeeding in the use of piles of mica; and the result fully justified his expectations.

Two piles of plates of mica were placed obliquely in the path of the rays, so that the inclination was that of the angle requisite for polarization. In such an arrangement, it is well known in one position of the second pile all light is stopped. The same was found to be true of heat; not only from flame, but even from non-luminous sources. This was not all; as in light, the interposition of a plate of crystal between the two parts of the apparatus just described, *restores*, or is said to *depolarize*, the light, so it was found to do with heat. On the principles of the undulatory theory this is explicable, and even subject to calculation in regard to light: by showing that a similar calculation will apply, Professor Forbes has rendered it in the highest degree probable that the same theory will hold good for heat; and has even pointed out the principle for calculating the lengths of the undulations necessary to be supposed, which he shows will be *greater* than those for light. This exactly accords with Melloni's result of their being less refrangible.

These are merely one or two of the long series of valuable results obtained by Professor Forbes. He has, since the publication of that paper, been carrying on the subject; but these investigations belong to those more complex properties, which it would be impracticable for us to attempt to explain, or even state intelligibly within our limits.

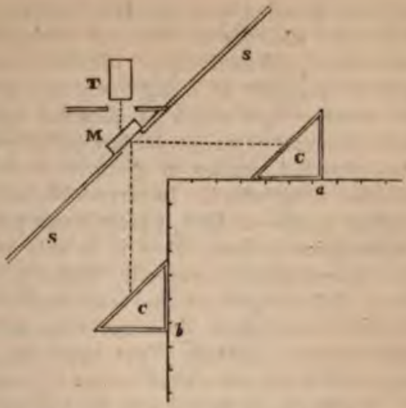
While speaking of researches carried on by the thermo-multiplier, we must not omit to mention that Dr. Hudson of Dublin has devised a very convenient arrangement for the use of that apparatus; and one which enables the experimenter decisively to free his results from any effect due to the secondary heat acquired and radiated again by the substance used as a screen; a source of error, of all others the most obviously essential to guard against. The annexed sketch represents the ground-plan of the whole arrangement; and will give a better notion of it than could be conveyed by any verbal description. We insert only the *essential* parts. The canister for holding hot water is of a prismatic form; and is placed alternately at (*a*) and (*b*) at the same distance. At (*a*) the heating influence of the medium or screen alone acts on the thermo-multiplier, at (*b*) that of the canister also.

T. The Thermo-multiplier.

M. The Medium for trial, inserted in

S. A brass screen.

C. The Canister in the two positions, *a* and *b*, presenting its surface at the same inclination.



With this apparatus, Dr. Hudson has repeated and verified many of Melloni's experiments; and has extended his scale of results, by showing a slight diathermancy in rock-crystal.

The thermo-multiplier has not by any means come into general use for experimental purposes; and in fact, there are many such purposes for which it is by no means essential; many fields of inquiry connected with heat still open, where we know so little, that we at present want rather more general indications than those of such extreme minuteness and accuracy, as to call for the use of Melloni's instrument. We shall now proceed to an account of some such researches, in which only ordinary thermometers were used.

Influence of Colour on Radiation.

The question as to the influence of the *colour*, and the state or *texture*, of the surface of bodies, on their power of absorbing or radiating heat, is one of the most curious, and at the same time, least accurately understood of any in the whole science of heat. Nay, it is one even beset with errors, and in reference to which a large portion of the speculations which have been broached, are vitiated by proceeding upon radically false analogies. This has arisen very much from the confusion of ideas concerning radiant heat; under which term several widely differing classes of phenomena or physical modes of action have been confounded together. We have already referred to one investigation on this subject; and the recent course of experiment has certainly tended considerably to dispel the mistake first adverted to, but still many fallacies arising out of it are apt to cling to the minds of experimenters.

The relation to colour, for instance, is quite manifest in regard to the heating influence of the sun's rays. Dark *coloured* bodies always receive most heat from the sun; and this, without reference to any other qualities or properties except some few whose effects are easily allowed for from known causes. This has been the conspiring result of all experimenters from the time of Boyle downwards.

But because this is true of the sun's rays, is it therefore true of those from other heating sources? this seems to have been assumed without so much as a question by many. It is, however, partly true: *luminous* hot bodies show some increase of effect on dark coloured bodies, but this is only true with regard to that *part* of their heat which has also (like the sun's rays) the property of being freely transmitted through glass. In either case it is the *light* which is absorbed by *dark coloured* bodies; and it seems to be an experimental law (whatever the cause may be), that *light when absorbed*, excites or gives out heat. This in the sun's rays constitutes the whole effect. In terrestrial hot bodies there is another accompanying species of heat, which is also the same as that which belongs to non-luminous bodies. Now it is with regard to this non-luminous heat (as it is somewhat incorrectly termed), that the main question arises. When *light* is present, we can readily conceive a relation to *colour*. When it is not, there does not seem a probable ground of analogy to afford us any conjecture. Yet some inquirers seem to have taken for granted that the cases were one and the same.

To this point then it was most necessary that experiment should be directed, and this was accordingly done with regard to surfaces in the first instance by Count Rumford and Sir J. Leslie, and subsequently by others. The distinction, indeed, between the cases just alluded to, was very little observed, and some sets of experiments such as those of De la Roche were considered to confirm the idea of a close analogy between the radiation which was called "luminous heat," and that which was not so. The distinction, however, was (as appears to us) conclusively established by the experiment of Professor Powell already referred to. More lately, Dr. Stark published a paper, in which he contends, not only for the influence of colour on radiant heat, without light, but even on odours, miasma, &c. (*Phil. Trans.*, 1833). To this Professor Powell replied in a paper in the *Edinb. New Phil. Journal*, October, 1834. The subject has attracted notice in America: and Professors Bache and Courtenay, of the University of Pennsylvania, have taken up the inquiry in a paper, published in the *Journal of the Franklin Institute*, November, 1835, in which, after an elaborate examination of the subject, they seem to accord very much with the views of the last-named writer.

Experiments of this kind are conducted by examining the influence of different coatings or pigments in favouring or retarding the radiation of heat. Now in all experiments by this method there is this radical difficulty: the same body is coated in succession, with substances differing in a variety of particulars; in colour, in smoothness, in chemical nature, in closeness or looseness of texture, in density, in conducting power, &c. &c., and, besides all this, in general differing in the actual quantity or mass which forms the coating; (unless, indeed, in some few cases, where the precaution of previously weighing them is resorted to, as in some of Sir J. Leslie's experiments). How then is it possible to be sure, among so many properties, *which* it is that constitutes the cause of increased radiation? It would seem necessary for this purpose (and perhaps it is the only way), to compare a vast number of substances, each agreeing in one property but widely differing in all the others. It is notorious that nothing like such an extensive comparison has ever been made, but we do not see how any positive conclusion can be arrived at without it. It is, indeed, fully established that the state and *texture of the surface has* a great influence. And the objection which was dwelt upon as most essential in Mr. Powell's paper above referred to was precisely this, that wherever there is a difference in colour there *must* be *either* a difference in the mechanical structure of the surface, *or* some new body is added or abstracted. We cannot infer, then, that it is not owing to these causes, instead of the colour, as such. But in point of fact several sets of experiments (as far as they go), seem to exhibit the magnitude of the effect as decidedly holding *no proportion* to the darkness of colour. This was the case with those of Sir J. Leslie, see *Reports of the British Association* (vol. i., p. 264.) The same thing also appears in the much more extensive series of results of Mr. Bache. He employed tin cylinders, filled with hot water, having a thermometer inserted through the top. They received various coatings; and the rates of cooling were compared. Among the results ranged in order of radiating power (*i. e.* more rapid cooling), though black coatings were usually among the highest, yet in many instances black and white are found alternating, and some black substances low on the list. Equally little could any reference to the order of other properties be traced; as roughness, chemical character, &c. In a word, it is a department of inquiry still open to research; and presents a rich field to any good and cautious experimenter who will in the first instance take the trouble fullly to consider all the circumstances of the case (including those we have just hinted at). We do not mean by this to disparage Mr. Bache's experiments, which, on the contrary, we prize most highly, as by far the most extensive series yet produced; and apparently conducted with the greatest attention to accuracy.

DR. HUDSON'S EXPERIMENTS.—SUPPOSED RADIATION OF COLD.

We have before referred to some ingenious experiments on certain points connected with radiant heat, which have been made by Dr. Hudson of Dublin. Those to which we now more especially refer, were made in continuation of some of Leslie's, and by methods somewhat similar. The question of the *radiation of cold* has been often agitated. We believe it is reducible to a very simple case of generally received principles. At least this appears to us to be true of all instances commonly adduced in support of it. We shall presently examine how far Dr. Hudson succeeds in substantiating it.

In these experiments, Dr. Hudson employed a differential thermometer, containing ether instead of sulphuric acid, as being more ready in its indications. The mirror was of a parabolic form, made of zinc, but having a hollow back, so that it could have its temperature altered by filling it with hot or cold liquids. The source of heat was a tin canister, having one of its sides coated with varnish, the others plain or metallic, and filled with hot water. The peculiarity of the apparatus then consisted in the means of regulating *the temperature of the reflector*. The following is a brief abstract of his results, on which we shall make our remarks as we proceed.

1st. The mirror and canister being both at the temperature of the air, no effect is produced either by the plain or coated surface. This is obvious; and is merely stated (we suppose), for comparison with what follows.

2nd. The canister alone being heated *above* the air, the varnished surface produced a greater effect on the focal bulb than the plain. The ratio in this instance was 12 : 1. This is a confirmation of the well-known results of Leslie and others.

3rd. The canister alone being cooled *below* the air, the varnished surface produced a greater *cooling* effect than the plain, in the same ratio.

The effect appears to us to have been simply this: the bulb being at the temperature of the air was a source of heat *relatively* to the canister. It consequently gave out its heat, as did also the mirror and all other substances near it, to restore the equilibrium of temperature.

4th. The *mirror was heated* to about 200° Fahrenheit. The canister at the temperature of the air: both bulbs were so placed as to be equally affected by the heat of the mirror; the canister displayed a cooling effect, the varnished side being most efficacious.

5th. The same arrangement was continued, except that the canister was heated 10° or 12° above the air, and placed at successively greater distances.

At near distances it acted as a cold body; as the distance increased, this effect diminished, and at a certain point ceased; on going beyond this, it acted as a hot body. In the first and last cases the relative effect of the surfaces was conspicuous.

In case 4, the canister abstracted heat *directly* from both bulbs. From that in the focus it also abstracted some indirectly by reflection, that is to say, both bulbs gave out their heat in all directions; but the heat from the focal bulb alone fell on the mirror so as to be reflected to the canister, when it was of course absorbed, and most rapidly by the coated surface. The mirror itself, being of a higher temperature, did not absorb the lower degree of heat which fell upon it. And this appears to us the important part of the experiment: it proves that *the reflection of heat by a mirror is not affected by its higher temperature.*

In case 5, this result is extended and varied by carrying the slightly-heated canister to certain distances. In the first instance it acted just as in the last experiment, being still *relatively* a *cold* body. It therefore abstracted heat from the bulb.

At a certain distance the canister ceased to abstract heat, or the bulb was not affected. Beyond this, the canister, being too far off to be heated, began to cool, that is to give out heat, which was reflected by the mirror upon the focal bulb; the coated surface of course gave it out most energetically.

6th. The bulb was kept cold by evaporation. The canister (we presume at the temperature of the air) produced no effect.

7th. The canister also being cooled below the air, but still 11° above the bulb, the cooling of the bulb went on more rapidly. We can only imagine, in this last case, that the slight excess of heat from the canister accelerated the evaporation. But these two last experiments are imperfectly described, and the author says the rapid evaporation of the ether renders them troublesome. We confess we cannot perceive to what conclusion they lead.

However, the general inference which the author deduces from the preceding results is, that no theory of *emission* of rays of heat can explain them, unless there be also rays of *cold*. He thinks it probable that Leslie's theory of *pulsations*, or some other modification of undulations, may account for the facts.

From what we have already said, it will be evident, that in our opinion the necessity for imagining rays of *cold*, is superseded by the simple considerations we have adduced in these cases, as well as in all the others when it has been so often referred to.

We confess we can see nothing in these particular instances which bears upon the question between the hypothesis of undulations and of emission. But as we incline to the former (especially on the grounds which Professor Forbes's experiments supply), we shall be anxious to see some more full developement of Dr. Hudson's arguments, which perhaps may disclose stronger reasons in support of the doctrine than any we collect from the notices as yet published.

The author has speculated with much ingenuity on another point of high interest, *the different radiating powers of different surfaces.* Understanding by the term "surface" some physical thickness of a substance, he conceives that the *radiating power* depends on the *capacity for heat* of the substance.

Two surfaces being at the same temperature, and in the same medium (of a lower temperature), may be considered to have the same tendency to attain the common temperature of the medium, and may, therefore, be expected to give off the *same* portion of their excess of *temperature*, and consequently quantities of *heat*, proportional to the capacities of the surfaces.

Formation of Ice in Rivers.

An interesting case in which the abstract principles of the science of heat are closely connected with the explanation of a remarkable class of facts in the economy of nature, is presented to us in the phenomenon of the formation of ice; taking place under very different conditions, according to the circumstances of the water as to rest or motion, and of the ground adjacent.

It is well known that in still water, ice always forms exclusively at the *surface*. If the mass of water be small, or its depth inconsiderable, the whole mass may become frozen; but ice *never* forms *first* at the *bottom*. The reason of this is equally well known. It depends on the very singular, but perfectly ascertained fact, (which in the present state of our knowledge stands out as so peculiar and anomalous an exception to the general principle, that bodies expand with the increase of heat,) that water, while it follows the ordinary law of condensation, down to a temperature of about 39° or 40° Fahrenheit; yet below that point, instead of continuing to increase in density, begins to expand again, which it continues to do down to 32°, when it becomes ice, and then expands still more.

The application of this principle in the formation of ice is manifest. When the water at the surface is cooled by the air down to 40°, it becomes denser, and therefore heavier than the rest, and consequently sinks: the portion which next rises to the surface, in its place undergoes in turn the same process: this continues till the whole is cooled down to 40°. The surface then begins to cool to a degree below 40°, but now this makes it lighter, and therefore keeps it at the top. Here it remains, then, till it is cooled to 32°, and converted into ice.

We have here supposed the water to be perfectly *still*. It may readily be allowed that any degree of motion might interfere with these arrangements; but to trace precisely *what* would be the effects, especially under the complicated conditions presented by rivers, may not be an easy task.

One result, however, will be readily understood. The rapid motion of a strong stream will cause the lighter and colder superficial portion more quickly to mix with the warmer part below, and especially when carried down by the agitation of eddies and irregular currents, will tend to cool down the whole mass much sooner than it can be effected in still water. Now, in point of fact, such differences are observed in regard to the formation of ice in still water and in rivers. It has been long a matter of observation, that in certain cases ice is formed at the *bottom* of rivers, though never at the bottom of still water. Some phenomena of this kind were observed and investigated, a few years ago, by Mr. Knight, who, in a paper in the *Phil. Trans.*, 1816, ascribes it merely to spiculæ of ice formed at the surface being carried down by eddies, and deposited on stones, and at the bottom of the river. The subject also attracted the attention of Mr. Mac Keevor and Mr. Eisdale, both of whom proposed theories: M. Arago, also, gave an excellent discussion of the whole question in the *Annuaire* of 1833. But our attention has been more immediately drawn to the subject at the present moment, by the appearance of a paper in the last part of the *Phil. Trans.*, (1835, part ii.,) entitled, "On the ice formed under peculiar circumstances at the bottom of running water." By the Rev. I. Farquharson, F.R.S., of Alford, Aberdeenshire.

The climate of Scotland affords greater facilities for the examination of this phenomenon, than have been enjoyed by previous observers. In the author's neighbourhood the appearance is familiarly known; and the particular kind of ice which is formed at the bottom of rivers, has received in Scotland the name of "ground-gru." It is characterized by forming not in the shape of *solid* ice, but of an irregular aggregation of filaments and crystals, growing up, as it were, from the stones, &c., at the bottom, and receiving successive additions as the frost continues. This fact of its mode of receiving accessions of matter is dwelt on by Mr. Farquharson, as opposed to the statements of some writers. Portions of it sometimes break up from the bottom, and float. Surface ice, however, forms also in certain parts of the stream, especially in the more still pools.

The author has observed the gru form only when the whole mass of the water was reduced nearly to the freezing point; and its occurrence is always preceded by a continuance of clear sky. Its increase is impeded during the day. When clouds intervene, it begins to be detached, and floats; often to such a degree as to choke the current and cause floods. The streams, however, speedily retire within their banks, and then become frozen over at the surface, none remaining at the bottom. This is called by the country people the *flitting* of the ice. The ground-gru occurs most frequently during a calm; though occasionally also with frosty wind. It has been affirmed by some writers to be *always* associated with hoar-frost: this is by no means the case.

Mr. Farquharson's observations were made on the rivers Don and Leochel, both consisting of alternations of pools and rapids. In the rapids the water is not more than a foot or two in depth, flowing over a rocky bed, and impeded by numerous fragments and large stones: in the pools it has much less velocity and rather greater depth, with a gravelly bottom.

On one occasion the author found the parts close to the piers and abutments of a bridge, free from gru, though in other parts it was plentiful. In some places, where the water was comparatively still, superficial ice formed, but no ground-gru. In one part of the bottom of a pool were tufts of water star-wort, (of a dark colour,) to which much gru was attached. In another part, stones at the bottom appeared to be coated with gru on the side opposed to the stream; but it was only a loose aggregation collected there, and not firmly attached. In a deep and still pool there was no gru. In a shallow rapid, going over a rocky and stony bed, abundance of gru was formed. Much of this was carried down and caught by the rocks below, as just described. In smaller streams the gru is comparatively more abundant. Near a steep bank there is very little formed. In a rapid, a dense fringe of Phalaris arundinacea seemed to keep the water free from gru under its shade. In a continued frost, the gru encroached upon the parts previously clear.

M. Arago, in the paper before referred to, (which was also translated in the *Edinb. New Phil. Journal*, vol xv., p. 128,) refers the phenomenon to the three following causes.

1st. The circumstance already adverted to, that in streams the rapidity of the current, especially in falls, carries down the colder water of the surface, and mixes it with that at the bottom; so that the deepest part is here colder than in still water.

2nd. The rough and pointed nature of the substances at the bottom favour the deposition of ice, in the same manner as similar asperities form the nuclei for crystallization in solutions of salts.

3rd. The motion of the stream near the bottom is retarded by friction; thus there is less impediment to the formation of the spiculæ of ice.

Mr. Farquharson admits that these causes are all in action, but denies their *sufficiency* to account for the *entire* phenomena. He contends they will not explain why ice is formed sometimes at the bottom and sometimes at the top of the water. All these three conditions are present in those cases when the ice forms at the surface. According to M. Arago, ice ought *always* to form at the bottom of running water. M. Arago, however, it should be recollected, does not offer his explanation as a *complete* theory: he admits that there are some points yet unexplained.

Passing over some other attempts at explanation, confessedly unsatisfactory, the author refers with commendation to the theory of Mr. Mac Keevor, though he conceives it wants a slight extension, which he in fact *proceeds to supply*.

This gentleman's principle is, that *heat radiates through water;* that the same laws prevail with respect to the influence of the state of the *surface* in promoting radiation, in water as in air. Consequently, the *rough* surface of the stones, gravel, &c., at the bottom, enable them to radiate heat, and cool fastest, and there the ground-gru forms.

M. Arago remarks briefly upon this theory, that the radiation through water is easily disproved; and that if true it would not account for the gru being found *only* in *running* water.

Mr. Farquharson contends for the fact of the radiation; for which he alleges certain experiments: *viz*, that the sun's rays collected by a burning lens act through water, and a glass globe of water itself collects them to a focus. He further insists on the clearness of the sky, which he found always associated with the formation of gru, as promoting the more rapid radiation; and the ice on the surface, as well as the shade of the bridge, the reeds, &c., intercepting it. But particularly he dwells upon the formation of the gru on the rough surfaces, and most especially on the *dark coloured* leaves, and stalks of weeds under water.

He remarks with perfect justice, that M. Arago's 1st condition will explain the distinction between still and running water; but that the same consideration refutes his own objection against Mr. Mac Keevor's theory: this objection may therefore be dismissed.

The main point, then, is the radiation of heat in water, and the experiments on which the author conceives it supported, as well as its relation to darkness of colour.

> "Every branch of the phenomenon," he observes in conclusion, "is of easy explanation when we admit the radiation; and among the rest, a circumstance to which I have yet made no reference, and that is the disappearance at the bottom of the water of the immense quantity of heat, 140° of Fahrenheit, which constitutes the caloric of fluidity disengaged, when water at 32° Fahrenheit is converted into ice at the same temperature."

And in the following passage his explanation is well summed up.

> "The answer to our original question then, is, that ice is formed sometimes on the surface of running water, and sometimes at the bottom, because frost sometimes takes place with a clouded sky, which is incompatible with radiation of heat from the bottom of the stream; and sometimes with a clear sky when that radiation takes place through the water, in the same manner as an experiment of Dr. Wells proves it, goes on under a like sky, through the atmosphere. The bottom is by this cooled down below the freezing point of water, before the water itself. Ice is formed on it, and its detachment by transmitted heat from below prevented as long as the radiation continues."

What, then, is the evidence adduced for the radiation of heat through water? that is, for its being able to take place from warmer surfaces under water? All that the author alleges amounts to this: that the *sun's rays* pass through water freely. This appears to us simply the same confusion of ideas between the *sun's rays* and the *heat*, from bodies which have an excess of temperature on the earth, to which we have already alluded. These have no doubt been very usually confounded under the vague, common appellation of radiant heat. The sun's rays pass freely through all transparent substances, solid or liquid, except that they suffer a trifling diminution, owing to the want of perfect transparency; and wherever they pass they carry their heating power with them.

That radiant heat from hot masses of matter (not luminous) cannot pass through any substance, solid, liquid, or even aëriform, without (at least) great diminution, is the conspiring result of *all* experimenters. That there are considerable differences in the *degree* of interception, was, as we have seen, the discovery of M. Melloni, who has detected extremely singular peculiarities in bodies in this respect. But amongst other results he has found most decisively that *water is wholly impermeable* to radiant heat, from terrestrial sources.

Again, with regard to the radiation being greatest from dark-coloured bodies. This is brought forward by Mr. Farquharson, as triumphantly established by the united experiments of a long list of philosophers, whom he names. The fact is, this is another common instance of confusion of ideas, into which many writers are apt to fall besides our author. It originates, as in the former instance, in the first place, from confounding the *sun's rays* with *radiant* heat; and in the next place, confounding the *texture* of the surface with its colour, in terrestrial hot bodies. We have above seen that the experiments of Sir J. Leslie are on this point of a tendency quite opposite to that in favour of which Mr. Farquharson cites them, and we believe as far as experiments at present go, that the radiating and absorbing powers of bodies in respect to *simple heat* follow *no proportion* at all to the darkness of colour; but wholly refer to the *texture* of the surface.

The author refers to the authority of Dr. Stark; but we have already observed that his reasonings have been, as we think, completely refuted.

We must content ourselves here by referring those readers who may feel an interest in the subject to the paper in question. We will merely observe, that in the particular instance to which Mr. Farquharson refers, *viz.*, the formation of gru upon the leaves and stalks of a dark-coloured plant, there is no proof whatever that it would not form equally well on similar substances of a light colour; and, in point of fact, the circumstance noticed by M. Arago appears quite sufficient to explain the appearance: *viz.*, that the stalks and leaves form nuclei of crystallization, upon which the deposition of the minute crystals or needles of ice commences, and round which others are immediately aggregated, exactly in the same way as the crystals in a solution of alum, or other salts, are well known to form by preference round any small substance introduced into the liquid.

The above considerations appear to us absolutely conclusive against the author's *theory*. Nevertheless, we think he has in this paper brought together a number of highly-curious new *facts* connected with the remarkable phenomenon to which it relates; and even these facts appear to us to furnish certain conditions which with one additional particular, go far towards supplying the deficiencies which are allowed to exist in the explanation.

1. In the first place, M. Arago's distinction of the descent of the warmer portions of water in the pools, and the greater or less prevention of it in the rapids, clearly explains how the water is in a condition

favourable to the production of ground-gru in the latter case, and not in the former.

2. The supposed *screening of the radiation* by surface-ice, the bridge, plants, &c., appears to us an instance of the non-causa pro causâ. The more true causes, in these instances, seem to us to be partly the shelter from wind, but also and principally the diminution of the force of eddies, &c., (by which the colder water is carried down,) in the parts near the piers, side-banks, &c. But,

3. There is another distinctive circumstance not adverted to either by the author, or other inquirers, but which appears to us the most efficacious of all others to the production of the phenomena. The adjacent ground, and the bed of the river, are first cooled down by a frost to a lower degree than the water; and thus the bottom will be rather colder than the incumbent stratum of water, even in the rapids, where it is soonest brought to an equilibrium of temperature; and this cooling down of the whole adjacent ground, of course, goes on most rapidly under a clear sky, by radiation at night; the ground in the bed of the river acquiring the same temperature, or nearly so, by lateral conduction.

Now solid rock is a much better *conductor* of heat than loose gravel, or sand, &c., and the former, it appears, composes the bed of the rapids, the latter of the pools. The former, therefore, conducts quickly away the slight excess of temperature in the running water of the rapids, and converts it into ground-gru. The latter conducts more slowly, and has also a greater degree of temperature to carry off; thus the gru does not form.

This appears to us to afford a very probable solution of the problem on well-established principles; though we are far from contending that there are not even yet some circumstances in the case requiring further investigation.

With respect to one point in the author's remarks before quoted, *viz*, the disappearance of the *immense quantity* of 140° of caloric of fluidity, however extraordinary it may appear, we do not see how *radiation* can explain the difficulty more than any other mode of carrying it off: for it must, surely, become sensible somewhere, by whatever means it be conveyed.

But we suspect the author has here perplexed himself by the not uncommon mystification in which this point has been involved. We will just state the case in a way which appears to us devoid of this ambiguity. Suppose 141 cubic inches of water at 32°. Suppose 1 cubic inch instantaneously converted into ice: then, 140° of heat are communicated from this cubic inch to the other 140. Thus, each of them gains 1° increase of temperature; or if a thermometer were put into the water it would indicate 33° as the temperature of the mass; and the water as quickly loses to the air and surrounding cold bodies all the trifling heat it thus gains, as fast as new ice forms, or faster.

ON THE NATURE, EVIDENCE, AND ADVANTAGES OF THE INDUCTIVE PHILOSOPHY.

II.

AT the present day, so common is the use of the term "Inductive Philosophy," that, it may be presumed, there are few persons who have not at least some apprehension of the sort of investigation which it is used to designate.

In its more general signification, this term is employed to describe the entire method of modern physical science, as peculiarly characterized by resting on the appeal to experiment and observation alone; and as contradistinguished from the scholastic systems, which proposed to reason down wards from abstract principles to natural laws and phenomena: the inductive, on the contrary, ascends from observed phenomena to general laws and abstract principles.

In its more precise and proper sense, however, "induction" is understood to signify the process of inferring and collecting general results, general facts, or "laws," from a number of particular instances, carefully established on actual experimental evidence. It is the nature of the process, thus designated, and the principles on which it is conducted, that we propose to explain and comment upon.

Now, it is clear that the first step in such a process, must be the collection and classification of a number of particular phenomena: the careful examination of a number of individual cases, in order to discover some common property or circumstance in which they all agree, amid many others in which they differ.

Does then the process consist merely in this, that we examine every individual of a class, or number of objects before us, and, finding each one to possess a particular property, affirm that as a common property of the class? This, certainly, would imply no exercise of reasoning, and would hardly be worthy the name of induction. We should be merely affirming a proposition for whose truth we had the direct evidence of our senses. Yet perhaps among cases even of this sort, there may exist much difference as to the extent and labour of the research we may have to go through, in detecting the one property, which is common to all the individual cases, and constitutes the characteristic by which we give them a common classification and a generic name: the affirmation of such a property, as applying to the whole class, is termed the general *law*, resulting from our induction.

The point in which all the examined instances agree, may, indeed, be manifest at first sight. But, again, it may be far otherwise; and though we have all the cases before us (especially if they be numerous), it may yet require no small labour and skill to succeed in tracing out what the property or circumstance is in which they all agree, amidst a variety of others in which they differ. The first case requires nothing further than the bare inspection of the instances. The latter may call forth much discriminative skill. The former is the work of the mere collector: the latter may involve that of the philosopher. But in any case other than the most immediately obvious, there is this to be remarked; and it is deserving of particular attention: it is almost certain that, in the first instance, the mind will conjecturally fix upon some property, which is imagined (whether correctly or not) most likely to be the common one sought, long before a complete examination of all the individuals has taken place.

Let us suppose, on the other hand, that we have *not all* the individual facts before us. We observe a certain number of them, and finding them agree in some property, we are almost invariably prone at once to infer, that all the rest possess it likewise. There is certainly a strong natural tendency in the human mind to do this, even upon very slight apparent grounds: to advance from individual facts to general conclusions,—to hazard inferences from the known to the unknown; or, as the case has been well described by Sir J. Herschel,

> "Such is the tendency of the human mind to speculation, that, on the least idea of an analogy between a few phenomena, it leaps forward, as it were, to a cause or law, to the temporary neglect of all the rest; so that, in fact, almost all our principal inductions must be regarded as a series of ascents and descents, and of conclusions from a few cases, *verified* by trial on many." (*Introd. Disc. in Nat. Philos.*, p. 165).

But with what reasonable confidence can we do this? For instance, suppose that feeling a number of balls in a bag, we take out a few, and, finding them white, infer that all the balls in the bag are white: is this a legitimate induction? Is it correct reasoning; is it not rather a most groundless presumption? Yet it may be asked, does it not possess all the characteristics of induction, as they have been laid down by many logical writers? For wherein does the case we have supposed, differ from their commonly cited example: "This, that, and the other loadstone, attracts iron,—therefore all loadstones do?" Or why is not the former of these instances as good reasoning as the latter?

The following distinction will perhaps assist us:—In the case of the balls, we cannot assign or imagine any conceivable reason why one should be white because others are so; any supposable connexion between the circumstance of the balls being together in the bag, and their colour. There is not even any tendency to fancy or expect it.

On the other hand, in the case of the loadstone, having observed the effect, in a few instances, we feel a natural tendency to imagine that the same magnetic property subsists whenever we perceive the same external characteristics. We cannot avoid being persuaded that there is a connexion between a certain darkness of colour, weight, hardness, texture, &c., by which we recognise the mineral, and a magnetic power, though, perhaps, we could not assign the slightest reason for it.

The only thing which seems at all to warrant the induction from a limited number of instances, is the reasonableness of such an intuitive persuasion. When, therefore, we have only a limited number of instances, which we can examine (and such is the case, in fact, in almost all physical inquiries), no inductive inferences can properly be made, unless we feel assured of some probable ground for expecting a common connexion to subsist between the individual cases. Can we, then, succeed in tracing any probable principle to which the existence of such a persuasion may be traced?—Can we analyze it up to any rational ground of belief? This is a most important point of our inquiry; and to it our *attention must* next be directed.

A POPULAR COURSE OF ASTRONOMY.

No. II.

THE word ASTRONOMY is derived from two Greek words—αστηρ, *a star*, and νομος, whose derivation is probably from νεμειν, signifying to *tend as a shepherd*, so that Astronomos means Shepherd of the Stars. The derivation from αστηρ, *a star*, and νομος, *a law*, is, according to the Greek idiom, the same with this.

Now let us suppose one of these shepherds of the stars, one of those *wise men*, those Chaldeans, who, as they tended their flocks by night, had their attention first called to the varied motions of the host of heaven, and thus beguiled the slowly revolving hours of their silent watching. Let us suppose one of them, for the first time, and uninstructed, to begin his observation of the stars, with which he will have seen the great vault of heaven studded. He will soon perceive all these stars to have a motion around him, rising from under the horizon to the East, and setting beneath it in the West; and the time occupied in this revolution to be nearly equal to that between sunrise and sunset. As these stars thus advance across the heavens, their places he will observe to be continually supplied by others, emerging, as it were, from some space beneath the horizon. These stars he will perceive not to move freely and independently of one another, but to partake in a *common* motion of the great canopy of the sky, round a certain point called its pole, whose position is due North of him, and whose height from the horizon may, in our latitude, be found by dividing the whole distance from it to the zenith, (that is the point immediately above our heads,) into ninety parts, and taking about fifty-one of them. All the stars will appear to describe as it were bands of the sky, equidistant from that Pole. Or the phenomenon may, perhaps, be better described by supposing the whole concavity of the heavens to turn round a line, drawn from him to that point, as a globe turns upon its axis.

He will soon, moreover, be convinced that this revolution is continued not only during the night, when he *sees* it, but during the day, when he does not; for, at the beginning of the next night he will find the very same stars, which on the preceding morning had disappeared, descending *westward*, now rising again *eastward*, having revolved during the day through some region unknown to him, and apparently beneath his feet, from the margin of the western to that of the eastern horizon.

He will, moreover, perceive that there are certain of the stars which disappear from him in the light of the sun at daybreak, which, nevertheless, can never go below the horizon; those, for instance, which lie immediately in the neighbourhood of the point called the pole, about which the whole turns. This point lying a considerable distance above the horizon, those stars, such as the constellation called Charles's Wain, or the Greater Bear, which are near it, and consequently describe small circles round it, can never pass beneath the horizon. The disappearance of these stars in the daytime, he will readily attribute to the greater brightness of the *daylight*, as he perceives artificial lights to be rendered scarcely visible in sunshine, and some of the smaller of the same stars to be invisible in the moonlight.

As of that half of the great sphere of the heavens which is above him, he perceives that there is a certain portion which by its rotation is never made to sink below the horizon; so of that half which is below him, he will conclude that there must be a certain portion which never rises above the horizon.

Hic vertex nobis semper sublimis at illum
Sub pedibus Styx atra videt manesque profundi.

His conclusion will be, that he stands in the centre of a great hollow sphere, on whose surface the stars are sprinkled. That this sphere moves continually round an axis, whose direction is inclined from him upwards to the pole; but that he can only see one half of it, or a hemisphere, as it is called, at once.

Impressed with this notion, let us suppose him to move from the position in which he first stood continually in any given direction, as for instance, westward. He will find that in each new position in which he places himself, he still seems to be in the centre of a sphere of the heavens in which similar stars appear, and which apparently revolves like the first, and round the same axis*. But the same sphere cannot have two centres. If then, in his first position, he was really in the centre of a sphere of the heavens, it follows that in his second position, he is as certainly in the centre of a second sphere, and that there are as many spheres of the heavens, as he can take up positions; containing all of them the same stars, and revolving all round axes similarly situated, and in the same direction, without interfering with one another. This is manifestly absurd, and he is soon forced on the conclusion, that go where he will he is still looking upon the same heavens and the same stars. And that in the appearance of a sphere, of which he seems to form the centre, there is some deception of the senses.

This view of the case his experience soon corroborates; he finds that he judges of the distances of remote objects from one another, by the number and variety of other objects, which lie within the range of sight between them,—the mind proceeding in the process as it were step by step; so that if there be no such other objects intervening between those which he observes, or if he can see none, he can form no idea of their relative distances. On a dark night, for instance, two lights, one of which is greatly further from him than the other, will appear at the *same* distance; since none of the intervening objects are visible to him. And for the same reason, the common distance at which they *appear* will be greatly different from the real distance of either of them. The stars then, although they appear to him all at the same distance, may, in point of fact, be all of them at very different distances, and scattered anywhere through the realms of space.

He will arrive at a similar conclusion from the appearance of the clouds. A little experience and observation will convince him that these e, in point of fact, huge masses of vapour floating at rest in the air, or vept over the earth's surface, in long irregular lines, by the winds. et these appear to him to form part of the concave of the heavens; all eir parts are pretty nearly at the same apparent distance from him. nd a long stream of them in motion to any point of the compass, instead moving in a right line, appears to wind around him in a circle.

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* That is, an axis going through the same point in the heavens—the same star, for instance—as before. The axis may be apparently inclined in a position different from its first position, but still intersect the heavens precisely as before.

Thus he clearly perceives himself to labour under an optical deception, which all the objects situated beyond a certain distance, are made appear to him to form part of the surface of a sphere of which he is the ntre. And it follows that the stars may be fixed in any way, however regularly in space, and however far off, and yet that go where he will, ey may always appear to him to lie upon the surface of a spherical vault ke that of the heavens.

Now let us suppose him to continue his journey in that western urse on which he first set out. Journeying on continually, he will find mself at last, to his great astonishment no doubt, in the very position om which he started. And in whatever direction, towards whatever int of the compass, he began his journey, provided he continue to move curately in that direction, he will find himself eventually to return to e same spot.

Now from this it is abundantly evident that the earth cannot be a ntinued *plane* or flat surface, for, if this were the case, the further he oved in the same direction, the further he would certainly always be om the point from which he started. It must, in fact, be a surface ithout limit or termination, and returning into itself, like that of a solid dy; such a surface being the only one from any point, in which any ne drawn always in the same direction will eventually return to that int again.

But perhaps it will be opposed to this argument that the journey pposed to have been taken has never, in point of fact, been made—that o one has ever set out from any place on the earth's surface, advanced ith his face always in the same direction, until he returned to that place -this is very true. But suppose the case of a number of different individuals respectively performing different portions of this journey, and all gistering and comparing their observations, and the case will be brought ery nearly, if not entirely, within the limit of that which has actually een effected.

Look at the matter, however, in another light, and the nautical xperience of every day presents us with a conclusive experiment. Suppose a vessel to set out from the port of London, and to sail so that her urse shall always be either due west, or due north, or south, or between ese points; or, in other words, let her course never be in any degree wards the eastern point of the compass. Now, it is clear that if the arth were a plane surface, or a surface of any kind which extended definitely, so as not to return into itself (enveloping a space within it), ship sailing thus could never return to its port again. She might wander n and on to eternity; but until she put about, and took an eastward urse, her voyage could never terminate in the haven whence she began t. Now this is what is *done continually*. Vessels are said to sail every ix *weeks, from the port of* Liverpool, on a south-western course to the latitude of Cape Horn; they then take a north-westerly course to Van Diemen's Land or New Holland; from thence they continue their voyage still in a north-westerly direction, probably to some of the islands of the Indian Archipelago, or to Bengal; and thence *again* sailing westward, but towards the *south*, they double the Cape of Good Hope, and, coming round northward, they find themselves approaching the region from which they set out; and at length make the very port from which they first sailed.

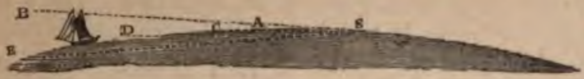
Here is a voyage, then, made continually towards the west, bringing the traveller again to his home; a line drawn continually towards the same *parts* returning into itself. Now such a line can only be drawn upon a *surface* which returns into itself and encloses a solid. This voyage would be utterly impossible, as every one may see in a moment, if the earth were a plane or any continued unlimited surface. Were this the case, wandering on continually westward, you would wander on for ever. Now voyages and journeys of this kind, have been made in every conceivable direction over the earth's surface, and always with the same result. It follows, therefore, that the earth's surface, in every direction, returns into itself; nowhere extending to infinity. It is not, for instance, a cylinder of an infinite length, but finite diameter, which we may gird *round* its surface, but not in the direction of its length. We do not live about the vertex of a paraboloid or hyperboloid, or about the summit of a cone, whose inferior surface spreads out to infinity, and whose base reposes in the abysses of space. The surface of the earth is finite in every direction, and the mass which it encloses is one wholly separate and detached from every other.

Now observe the importance of the fact which we have thus arrived at. Since the earth is a mass, thus separate and detached from any other, *it reposes or rests upon no other*, as we perceive bodies here to rest upon one another. We have great difficulty in conceiving this fact, which is nevertheless demonstratively proved: it appears to us utterly impossible, and opposed to daily experience, that anything should repose, and yet be *unsupported*, without any foot, or pedestal, or plane to rest upon. All this is easily explained, but it does not belong to this part of our subject to explain it. The reader must for the present content himself with the broad fact, which has been absolutely and completely proved to him, that the earth is a finite mass, bounded everywhere by a surface which returns into itself; nowhere reposing or resting upon any other mass, but, by some means or other, poised in mid space, by an indwelling energy, or power, or some unseen influence from without. It is a solid mass,—a lump of matter; or it is a hollow mass, having a surface like a solid.

The argument on which this conclusion is founded is *perfect;* it is an absolute demonstration.

There is a common illustration of the fact that the earth is not, as it seems to be, a *flat* surface, but that it is curved, drawn from the manner in which a vessel first becomes visible, as it approaches the shore from a distant voyage, or disappears as it leaves it; it is very valuable, inasmuch as any one may repeat the observation for himself, by going only as far as the sea-coast.

If the surface of the earth be curved, as represented in the figure, and the eye of an observer be placed at S, it is clear that to see any object



placed elsewhere on it, as at C, D, or E, he must look through the mass of the earth in the direction of lines drawn from S to these points. Objects situated at C and D and E would therefore be invisible to a spectator at S; and, in fact, all objects situated beneath a line, SB, drawn from S, touching the surface of the mass in the point S, whilst all objects above that line would be visible to him. Thus, a high-masted ship approaching him, in the direction EDCBA, or leaving him in the direction ACDE, would, as long as the whole of it lay beneath this line, be invisible; and the first part of it which found its way above this line, or the last part which remained above it, being the topmast, would be the first to appear, or the last to disappear; and thus, as it approached, the different parts of it would, in order, rise into sight, until the whole of it appearing above the line SB, would become visible; or, if it was leaving him, sinking more and more beneath that line, as it receded, the whole would become gradually invisible.

Now this is precisely what any person who made the observation would find to be the case, wherever he made it on the earth's surface.

The appearance represented in the cut, is that which a ship always offers as it goes out of sight on a distant voyage.

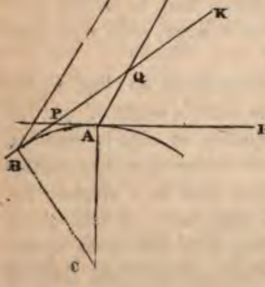
He would also perceive that, if the earth were a perfect plane, or flat, not the topmast, but the stern, must, of necessity, be the last part visible. Here again, then, he positively concludes that the surface of the earth is a curved surface.

There is yet another proof of the curved form of the earth, which, like the first given, is a complete and absolute demonstration of its continued surface and finite dimensions; and which, although in introducing it, a fact will be *twice demonstrated* (which is contrary to the rules of sound reasoning), it will yet be desirable to give, because the reader will be led on by it yet another step in investigating what is the real and accurate form of the earth's surface, and will be further enabled to fix its *dimensions*, as well as its form.

If an observer set out from any northern latitude, and travel southward, he will observe the *altitudes* of the stars to the north of him, or their heights above the horizon, continually to diminish. They will appear through a greater angle than when made to roll over *an inch* at the thicker extremity, or on either of the sides of the egg. Now, the *angular* motion of the horizon of an observer, travelling over the earth's surface, may be ascertained by its angular approach to any fixed star; and it is found, by numerous actual observations of this kind, that when the horizon of an observer is made, in any two different places on the earth's surface, by a change of the same distance in his position, to roll over the same space (say sixty-nine miles), due north or south; it revolves also through *very nearly* the same angle, approaching or receding from any fixed star by the same angular quantity. It follows then, with absolute certainty, that the earth is very nearly a sphere. Very nearly, because the angle through which the horizon thus rolls, in any two different places, is not *exactly* the same; it is slightly greater towards the equator than at the poles. The earth is, therefore, slightly more curved at the equator than at the poles. Its form, in fact, is somewhat that of an orange: the polar regions corresponding to the parts about the extremities of the shorter diameter of the orange.

Let us suppose our observer, by this time, to have acquired sufficient knowledge of geometry, to perceive that the angle through which his horizon revolves between any two stations is, in point of fact, the same as the angle made between two lines drawn from those two stations to the earth's centre*; and very little knowledge of geometry is necessary for this purpose. Knowing this fact, it will at once suggest itself to him, that he may determine the complete girth or circumference of the earth by a very simple process. He has only to move on until he has made his horizon to revolve through any given portion, say to the 360th part of a *complete* revolution. He then knows that he has also made the line, drawn from him to the earth's centre, to revolve through the 360th part of a complete revolution. The distance between his two stations is, therefore, the 360th part of the earth's circumference. Let him then measure this distance, and multiply it by 360, and he will get at once the complete girth of the earth; this he will thus find to be about 25,000 miles. Now, knowing the circumference of a great circle of the earth, he can find the diameter of the earth and its solidity.

The polar diameter of the earth is thus found to be 7899 miles, and its equatorial diameter 7925 miles.



* Thus let A and B be two positions of the observer, and let AC and BC be perpendiculars to the horizons at those points which, if the earth were accurately a sphere, would meet in its centre. Let SA and TB be straight lines, drawn from a star to an observer, at the points A and B; these lines are parallel, since the star is infinitely distant. Let AH and BK be the horizons at A and B; then are SAH and TBK the altitudes of the star, as seen from A and B; and the descent, or sinking of the star, by reason of the motion of the observer from A to B, is the difference of these altitudes. Now since AS and BT are parallel, therefore TBK is equal to BQA, and the difference of BQA and SAH is QPA; therefore the difference of TBK and SAH is QPA. QPA is therefore the difference of the altitudes of the star at the two points of observation. Now QPA = ACB ∴ &c.

SKETCHES *FROM LIFE* OF SOME EMINENT FOREIGN SCIENTIFIC LECTURERS.

We suspect that *Blackwood's Edinburgh Magazine* is seldom consulted by the ardent student of natural or applied science, and that if No. CCXLV of that powerfully written periodical should accidentally offer its table of contents to his observation, "Paris Mornings on the Left Bank of the Seine," would scarcely, as a title, induce him to suppose that anything in harmony with his pursuits would be found there. It is more than probable that he would not, agreeably to its instructions, apply to page 296 for further information. Under this impression, and anxious that this class of our readers may be able to enjoy a few *minutes* of these brilliant *mornings*, we have made some extracts. We intend that these should convey some idea of the energetic action of the moral apparatus with which the lecture-rooms of Paris are so richly furnished.

In perusing them we are sure that the natural and blameless curiosity which urges us to become acquainted with the persons and manners of those whom we admire and honour, will be greatly interested. The venerable Christopher, though wandering among Institutions whose legal protector is "a citizen king," forgets, at least in the specimens we propose to give, "the fierce democracy" against which he has sworn eternal war, and mingling with the doctors of the Sorbonne, puts on the professor's robe, and, with his usual vigour, lectures on those who have been all their lives lecturing to others. "Beginning at the beginning," he makes the usual flourish—chronological, antiquarian, and learned, but soon seizing the crayon, he sketches the locality and the furniture of the antique hall in which he stands, and then with his wonted breadth, and more or less of finish, he selects a head, and dashes off a "study from nature." Eminently successful, both in his subjects and his effects, he thus presents a series of highly characteristic portraits of some of the greatest men in the French capital. The *force* of these sketches will be universally acknowledged. We know that their *fidelity* must be assented to by all who have had the enviable means of judging.

We can fancy ourselves once more in the vast and shabby amphitheatre of the Sorbonne, sitting upon one of its long and dirty benches, surrounded by a host of enthusiastic students, and waiting the arrival of the lecturer; though the walls may be ill painted, and the ceiling smoky, and the furniture of the roughest workmanship, yet there is about the whole so perfect an air of business, that every one who enters, at once feels that he is in no place for display and ornament, but in a thorough school for the investigation of science. In front is the long table with its load of apparatus all ready for use, whilst against the distant wall is the gigantic slab on which the professor has chalked out his illustrations,—and which, before his entry, the student is now busily copying into his note-book. This slab has, indeed, been an ancient and a noble herald of information and recorder of discovery, and is well worthy of the honourable mention which has been made of him. We think that he must have leapt in his old grooves, in spite of his "gravity," absolute and specific, at the "*Salve*" of the following gratulation.

"Hail! old Slate of Rimorgne! what changes in the face of science have been represented on your face, since you were first brought from your dark cold bed, with marl for a mattress, and red sand-stone for a counterpane! Many a learned conjecture respecting your own bodily formation has been hazarded in your very presence; yourself the theme of discussions, on which your own revelations would have been conclusive, had nature permitted the unfolding of subterranean secrets. An unnatural conspiracy, truly, was that of brother minerals, charcoal, sulphur, and nitre, which betrayed you into the power of man, and blew up your early attachments! What has not been dared and done in those *quartiers bruyans** of Paris, from which the river happily divides us, most venerable Schistus, since you were first smoothed and squared, mounted and framed? All that Blainville quietly imparts, or Mirbel more strikingly exhibits, has been confided to you! Where be the mysteries that you have not assisted to simplify? How oft has the *nisus formativus*† of animal, vegetable, and mineral existence been canvassed on your impartial square! How often has your intelligent panel telegraphed to the distant benches of the large audience, not only all the discoveries, but all the Pseudo-Eurekas‡ of the learned! The hand of a Cuvier has lately swept over your plain! the creative touch of a Jussieu has made fair flowers spring up from your unpromising soil, amidst the winds of March! Myriads of insects, marshalled by St. Hilaire, have crept over your *tableau vivant*§! Fishes have I seen, how often! in all the audacity of tail and fin, sporting upon your black sea! Here the mountain has been bidden to rise by some daring geologist; there the continent has been abridged by encroaching waters;—sponged away while he yet spake! Comets have displayed their streaming banners, and clustering stars have sown their galaxy on that dark firmament! Nor is there, in fine, anything susceptible either of exhibition or of demonstration, of diagram or picture, which has not furnished its contribution, and been *octroyed*‖ on that most fertile field, which produces, often on one day, its triple and quadruple harvests."

Soon after the Veteran proceeds to the professors themselves. We shall pass over those of metaphysics, history, belles-lettres, &c., and take only such as are occupied with physics. Here is the lecturer on natural history,

BLAINVILLE.

"What one cannot fail, I think, to be most struck with at the Sorbonne, is that unambitious, unrhetorical manner cultivated by those enviable teachers, who have devoted themselves, their talents, and sometimes even their fortunes, to the study of 'Nature;'—who interpret her laws without ostentation, and present her in such advantageous simplicity to minds not yet conversant with her charms. We have one Faraday, the French have more than one. Is it possible, I have sometimes asked myself, that a naturalist can really be peevish? Let them talk of you, Monsieur Blainville, as the most ill-tempered personage that ever exhibited the fang of a rattlesnake or the thorny *lophoderme*¶ of a *centronate*** or stickleback! but we have had ample means of ascertaining your indulgence to the persons who approach you for information, and are convinced that, *au contraire*, you are essentially a good-humoured and an excellent specimen of our order of mammalia; we have attended your lectures regularly, and have not only seen specimens of all your favourite fish, but can attest with what wonderful

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* *Turbule* [illegible] points of o[illegible]rigin.

‡ Mares' nests!

§ *Vivid pictu* [illegible]

¶ Hairy skin.

** A kind of fishes.

sleight of hand your rapid chalk can gird on the armour of an *Ophites*, give its Highland cheekbone to the gurnard, spread its soft pearly coating over the mackerel, or exhibit upon the ever-changing field of the large Slate, the wonderful apparatus of the *gymnotus**! We have also seen your book upon shells, or rather upon *malacology*†, which, while it displays the deepest research, contains abounding proof that classifications may be founded on philosophy. Yes! there are higher exercises of the psychological functions, even in the study of this branch of science, than pinning a butterfly in a grove of cork, or drowning a beetle in alcohol. Surely there is nothing meaner (short of being positively vicious) than seeing some old collector, thumbing his dirty copy of Latreille, conning over, to him, hard Greek names, counting the segments in the corslet of a fly, or noting the subdivisions of the tarsus of a flea's foot. The study of nature, if this be such, so pursued, and pursued no further, does positive harm, by bringing discredit upon the science of natural history, and debasing the philosopher down to a mere accumulator of specimens.

"Monsieur Blainville is about 55, evidently of a happy *crasis*‡, indefatigable and enthusiastic now, as they say he was twenty years ago, and never tired or tiresome, though he lectures frequently for two hours at a time. From Monsieur Blainville I have learnt to be no longer astonished at the velocity of the swimming powers of the mackerel; he has instructed me that all the *Scombri*§ have this property of outstripping most of their neighbours in speed, and that this facility of motion (in which they excel all other fish) depends on the bifurcation of their tails. The Tunny and Dorax (of this family) swim at the rate of eight leagues per hour! and the fleetest fliers among birds have this same peculiarity. 'The swallow will immediately occur, and thus a very interesting analogy is established between birds and fishes.'

"The '*erectus in terga sudes*‖' of Juvenal had perplexed commentators; but Blainville interprets the poet and the passage, by showing that the *rhombus* actually has this property of erecting his bristles, and in a way which is truly remarkable.

"'In birds, reptiles and insects, there are some which have been falsely called *apteroids*¶, or *apods***; for they possess in concealment the members which their name declares them to want; and this analogy also extends to fish, some of which have been falsely supposed *apods* in consequence of their ventral fin being concealed within their body.'

"'All fish have what are called stones in their ears; in the *scienæ*, these stones are of a very large size, and are three in number. Of the *percidæ*, which frequent rocks, and are common at Dieppe and along that coast, I show you here the *apistos*, or, as he is emphatically called, sting-fish, whose large supply of spines is probably intended to protect him from being driven against the rocks by the lashing of the waves—just as the rower pushes out his oar or his boat-pole for the same purpose. As the swim-bladder is found very large in some fish which swim little, and small in others that are expert swimmers, and does not exist at all in the mackerel, which is the fleetest swimmer we know, the swim-bladder must answer other and more important ends, than the one from which it derives its name.'"

The next sketch is that of M. Pouillet, the professor of physics.

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* Electrical eel. † Treatise on soft-shelled fishes. ‡ Temperament.
§ A kind of fishes. ‖ Prickles bristling on its back. ¶ Wingless.
** Without feet or fins.

POUILLET.

"Monsieur Pouillet has (with the single exception of Dumas) the largest class which the Sorbonne exhibits; the number of his pupils cannot be less than 600; indeed, it is probably considerably above this calculation. He seems to be about fifty, has keen hazel eyes, and a pleasing physiognomy, and lectures with that perfect ease, which none but a man thoroughly possessed of, and by, his subject, can assume. As the following passages were then new to me, some of them may probably be so to others.

"'A contracted muscle, or one in the act to contract, increases prodigiously in force, by the closer approximation of its molecules; this is partly because as it diminishes in length it necessarily increases in thickness; but the difference of cohesive strength or tension between the living and the dead muscles depends mostly on the vessels which pervade the former being full during life, whereas after death they are of course empty; while they are full, the force applied to the muscle acts equally upon all its fibres, and the tension of all parts being equal, the force is equally divided; thus the strength of a wet cord or cable is far greater than that of a dry one of the same thickness, because the penetrating moisture gives an equality of tension to its fibres.'

"'The power of adhesion noticed between two bodies placed in juxtaposition, with a layer of fluid between, is not attributable, as commonly stated, to the partiality of atmospheric pressure; this is proved by putting two moistened surfaces of glass in contact, fastening a weight to one, and then placing them in *vacuo*, the weight will be found to remain suspended; that is, the thin layer of fluid interposed has a double adhesion, by its two surfaces, to the two solid surfaces with which it is in contact, and which it thus chains together. The action of all glues and pastes is of this nature, and is twofold. First, they act merely like water interposed between the two flat plates of glass, filling up the interstices of the bodies to be united, and so multiply the points of adhesive contact, and when they dry, the bond of adhesion becomes solid and confirmed. So, for I like to apply knowledge to knowledge, in what Hunter calls union by the first 'intention' (and what Aretæus had spoken of in almost identical phrase—κατα πρωτον σκοπον*), lymph (which is a fluid cement) is first effused; afterwards, as the liquid parts are becoming absorbed, the lips of the divided surfaces are more nearly approximated by the constantly attenuating layer, till they are brought within the sphere of mutual and permanent attraction.'"

This account of M. Pouillet is not so full as we could wish it. He is, however, a man so simple in his manners, and beset with so few eccentricities of character, that it is very difficult to give any marked outline of his manner, notwithstanding that he is the most popular, if not the best, lecturer in Paris. His characteristics, however, are fluency of language, clearness and occasional eloquence of description, zeal in his cause, ease of manner, and constantly animated mode of expressing himself. He is certainly the finest talker we ever heard, and the most perfect master of the crayon we ever saw. His outlines on the "old slate" rival in precision those of mathematical instruments. His principal attraction, however, is that perfect absence of self-esteem and pompousness of manner, so peculiar to lecturers of this country. M. Pouillet is the antipodes of a "Doctrinaire;" and the object of his experiments and lectures appears to be less to instruct his class than to advance or investigate those branches of science to which they refer. The lecturer and his pupils gradually forget their different relations, and work together as labourers in one calling,—fellow-gleaners in the same field of science. The degree of sympathy which exists between them is surprising; and the smile of M. Pouillet, his earnestness, his delight in the success of his experiments, his admiration of the grand laws of nature—all find an echo at once in the breasts of his auditory, for he and his class are identical and the same.

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* At first sight.

This state of things is to be found to the same extent in no other class-room in Paris; and though it is said that there are other lecturers more profound, we think there are none from whom so much is gleaned, and that too so agreeably, as from M. Pouillet.

We shall have nothing to add to the limning of the professor of Vegetable Physiology,

MIRBEL.

"Mirbel is a very clear, unaffected lecturer, a most worthy colleague of Blainville and Jussieu; he looks like one of his own dried plants, perfect in its anatomy, but sapless withal. He adopts a quiet conversational manner, and considers extreme accuracy in what he states to be so imperative, that if he occasionally forgets himself, he always apprizes his class of the error.

"More fond of accurately ascertaining natural phenomena, than of imagining theories to explain their laws, or make them more striking or attractive, he truly observes, that almost any fact in nature is more beautiful, than the most excursive fancy can render it. His unreserved manner of stating his own changes of opinion, makes it evident that he is of the few who do not fall into the 'error of concluding from partial views or first ideas, and then assuming, as our doctors do, a purely hypothetical generalization as an axiom of science.'

"The procedure by which the bark of trees splits and peels off from the stem, was thus explained—the green envelope or bark (which, by the way, owes its colour to a minute portion of green pigment contained in some very minute utricles of the vegetable texture) begins to split so soon as it has ceased to form these utricles in sufficient quantity to meet the exigency of the increasing diameter of the tree. Thus, in some trees, a very few years are found sufficient to effect many and large lacerations of the bark.

"Respecting the well-known fact in rural economy, of the necessity of changing crops—of not requiring the same land to do the same thing twice in succession, the following excellent remarks were made. 'Every farmer knows that he is obliged to vary his crops, nor does he ever think of exacting corn for two successive years from the same piece of land; but few are probably aware of the many explanations which have been proposed, to explain this apparent caprice in the earth. When it had been ascertained that any pivoting plant (as turnips for example) flourished upon the soil where the year before the *lætæ segetes*[*] had waved their yellow corn, this was supposed, by some writers on agriculture, to result from the radicles of this class of plants having a power to make their way through the already impoverished superficial layers, and striking deeper into a virgin soil where the power of sustentation was yet unimpaired. To this hypothesis it may be objected, that were the elements of nutrition and growth contained in the earth, and these all that plants required for their support, then it would be sufficient to manure the corn-field of the year preceding, to make it capable of a second year's crop. This, however, is contradicted by experience. Pictet's account is not more satisfactory. This writer supposed that the circumstance of turnips succeeding corn (or of the succession of crops to each other generally) was probably owing to different plants requiring and withdrawing, not the same but different elements, the supply of any of which being limited, the earth is amerced of some new and sustaining principle by every succeeding crop. This explanation, however, assumes that plants have a power of selecting their nutritive materials, an assumption not only hypothetical, but wholly contradicted by experience; for plants are observed to take up indifferently all substances soluble in water. The speculation of Decandolle is a third unsuccessful attempt, viz.:—that the excretions from plants during their growth may act as poisons to the earth, and, after a certain time, so injure it, as to prevent the further growth of a plant that may have recently flourished there. Such excretions he supposes to be emanations from the root, the remains of those juices which the earth and air conjointly supply, and upon which, in reality, the plant exists. But against even the very fact mentioned by Decandolle in confirmation of his opinion, that opium strewed upon the ground kills plants and renders the soil henceforth unproductive, we may quote the much more apposite fact, that trees (and why not therefore, *à fortiori*, corns and grasses) grow and flourish for entire centuries in the midst of excretions from their roots.' Mirbel's own explanation is simple, and we think satisfactory. Plants require other elements for their support, besides the elements of assimilation, and cannot thrive without them—for instance, there is silex in the cane, and there is lime in certain plants whose organization could not be complete without it. The quantity of any such foreign ingredients in a plant is generally very small; but the necessity for it may be presumed absolute. Plants cannot be constituted unless all the materials they require be furnished to them; and indeed the same observation will apply to animals; deprive a hen of lime, her eggs will have no shell; deprive animals generally of salt, and you ruin their power of digestion; deprive the earth then of its soda, and you must supply its place by potash; for salts are the excitants of the growth of plants, and of the clovers in a very remarkable manner. The smallest quantity of sea-salt has frequently been found to effect wonders in vegetation. But the spontaneous formation of any of these salts is the result of very slow chemical changes, which have been at work for centuries, and when the natural and very limited supply is exhausted (as it soon will be, if the earth be forced to give her increase), the corn of every succeeding year deteriorates, the field looks shabby, becomes chlorotic, and pines away; but allow the corn to fall where it grew, the earth will re-assume the salts extracted from it during such growth, and the same grain will continue to flourish indefinitely —in short, a peck of salt is worth a ton of manure, and it is to the understanding of this fact that we may attribute the luxuriance of the environs of Paris, where the soil is naturally of the poorest kind, but is made, by this simple addition, to yield its unequalled produce, and to fill its flower and its fruit markets with plenteousness."

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[*] Gay crops.

M. Thenard, *Le Baron!* is one of the professors of chemistry, and M. Dumas is another. They are thus placed in juxtaposition.

"Two *savans*, the most unlike in their manner, their mode of lecturing, their voice, their *ensemble*, but passing for equally good chemists, give *alternate courses*, and instruct unusually large classes."

We shall separate the pair, for the purpose of extending the first, at least, to somewhat of a whole length. He is too remarkable as an individual to be blended or associated with any other; and therefore, though it may be with a feeble pencil, we shall attempt to fill in some of the blanks left on the canvass by our great master. The following is all he has put in of

THENARD.

"Monsieur Thenard is an elderly person, and of somewhat unpleasant *accueil**. I understand, however, that he (like many others) finds it convenient to have two manners, and I here speak only of his every day one of conducting himself to strangers. He appears to pride himself more upon his peerage than upon his chemistry; is notoriously choleric, and detonates upon the object nearest to him like one of his own chlorides. As wordy as Isæus†, and having a good wind, he can scold indefinitely. He speaks loud and without hesitation, but often drops his voice at the most important word of his sentence, leaving you an enigma to make out, instead of a truth to put by. His recommendation to carry a piece of quick-lime with you in case of cold, and no fire accessible, was at that time a novelty. 'You can get cold water,' he observed, 'everywhere, and your lime has only to be slaked in any covered vessel, to afford a really excellent foot-warmer—indeed you may cook your cutlet on a metal plate, under which lime is slaked.'"

Thinking this is incomplete, we have ventured to put in the following detail. The same remark may be made as to his appearance as has been applied to his *accueil*, for no one can look on his broad face, black, curly, long hair, capacious forehead, quick, penetrating, and cunning eye, long upper-lip and wide mouth, together with his squat and generally dirty person, and pronounce it to be otherwise than unpleasant. Every observer, however, would at once set him down as a most active-minded, but odd-looking, personage, possessed of a great deal of very strong sense, but in whose character irritability, and a love of fun, are the marked peculiarities.

He is a very different lecturer to M. Pouillet; he is more familiar with his pupils, and there is more *plaisanterie* passing between them; his style is extremely amusing and facetious, and frequently his lecture cannot proceed for minutes, owing to the convulsed laughter of his pupils. He has gained the name, amongst the students, of "*Le Farceur de la Sorbonne*‡;" and his theatre is filled almost as much by those who go to enjoy his jokes, as those who go to profit by his lectures. His face is watched most intently; and when his eye brightens, and his head is archly turned on one side, the joke is known to be on its way, and the pupils hail its coming with grins of merriment, or with peals of laughter. M. Thenard's last piece of buffoonery is as much the subject of conversation at the Sorbonne, as was "Lord Norbury's last" in the Irish courts of justice.

M. Thenard is, therefore, an amusing companion for an hour and a half, but we doubt whether chemistry is benefitted by his witticisms. On many occasions, he descends from the philosopher to the buffoon, and draws his class down with him; whereas M. Pouillet, by being the earnest minister of science, raises his hearers to the level of the philosopher. The one creates a thirst for knowledge in the student, the other jokes for his leisure hours.

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* Manner of receiving a person.

† The master of Demosthenes; he composed sixty-four orations.

‡ The *Jester of the Sorbonne*.

His style is full of light and shade, and is more stormy and abrupt than that of any one we ever heard. He is excessively fond of placing himself in a dilemma—of inquiring of his class how he is to extricate himself,—suggesting means that he knows will not answer, and then, after playing with and enjoying the embarrassment of his audience, he jumps unexpectedly upon the remedy, and laughs at the surprise and ignorance of his pupils. In his tones, in his reasonings, and in his experiments, his sole object seems to be—effect; he delights more in astonishing than convincing; and, as might be expected from such a character, he is vastly rapid in his movements, and impatient in his experiments. It is then that the irritability of his disposition most shows itself. We recollect once witnessing an experiment, during which he had thrown himself into a dilemma, like the one we have already mentioned; and after suggesting one ineffectual remedy after another, he at last, with a view to take his audience by surprise, and to liberate himself at once, exclaims, "*Mais, nous n'avons pas encore essayé avec de l'acide sulphurique.*" ["But we have not tried sulphuric acid yet."] The words were hardly uttered, before he bounces from one end of his table to the other, and with a self-satisfied grin upon his countenance, seizes upon the bottle,—when lo and behold—it is empty! His fury at the disappointment cannot be conceived, much less described. Turning upon M. Barouel, his assistant, with the spring of a tiger, and raising up both his hands above his head, he screams at the top of his voice, "*Mon Dieu, Mon Dieu, Monsieur Barouel, où est l'acide sulphurique? Où est votre tête, Monsieur Barouel. Croyez vous qu'on peut faire des expériences sans tête et sans acide sulphurique?*" ["Good Heavens, Good Heavens, M. Barouel where is the sulphuric acid? Where are your brains, M. Barouel? Do you think that any one can make experiments without brains and—without sulphuric acid?"] M. Barouel explains to him that the bottle was full at the commencement of the lecture, and that he had himself emptied it in his experiments. The class roars with laughter, and M. Thenard more than any one else. When silence is re-established he draws M. Barouel forwards, clapping him gently on the back, and looking archly at his class, he says, "*C'est un bon enfant celui-ci, Messieurs* (M. Barouel is at least fifty years old,) *c'est un excellent enfant. Mais il perd la tête quelquefois, et quelquefois aussi de l'acide sulphurique; et cette fois-ci j'ai cru qu'il avait perdu tous les deux.*" ["He is a capital boy, gentlemen, an excellent lad; but he sometimes loses his brains, and sometimes—the sulphuric acid; this time I really thought he had lost both!"] Another laugh from his class; more acid is brought, and he continues his experiment.

In his laboratory, or after his lecture, he is very affable with his pupils, and always encourages them to ask him for explanations of any parts of his lectures which they may not have understood.

The *pendant** to M. Thenard is thus delineated.

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* Companion-picture.

DUMAS

"Dumas is a perfect gentleman in his manners, and wears his ribbons gracefully; his lectures are minute, without being tedious in detail. I consider him to be a very first-rate expounder of the doctrines of affinities. He has a very large and attentive class, and does not glare around him like some dirty and mischievous hyæna, nor affect the style of a rhetorician, while he is adding an oxide of antimony to a saturated solution of potash. His lectures abound in the most interesting facts; his experiments always succeed; what he presents to you unostentatiously, you remember easily, provided you are fortunate enough to hear it. Of the miscellaneous application of chemistry to arts, he indulges his class with an occasional and judicious selection, for it is clear that a course of chemistry should never merge into dissertations on dyeing and calico-printing.

"Oxide of lead, water, and any fat substance, duly mixed and heated, will produce *a* soap; but the same substance treated with soda or potash is preferable. The manufacturers of Marseilles (which supplies almost all France with its soap) generally employ potash, though soda is sometimes used. Soaps are true salts; that is, they have an alkaline base united with one of three acids, either the oleïc, margaric, or cetic; the first being contained in oils, the second in animal fats, the third in spermaceti. Soaps from which the *glycerine** has not been extracted, spoil in a short time, and therefore it is indispensable to effect a separation. Fortunately, this separation is easily managed; nothing more being required for the purpose than to mix sea-water with the oil which has become pasty in its progress towards perfect soap. A great deal of water (nearly 50 per cent) remains in soap after it is solidified. The different colours of soap are produced in different ways; protoxide of iron makes it blue; nut-galls black; a green colour is formed by indigo; transparent soaps are made by solution in alcohol; soap for washing in sea water (which has not yet been made in France) contains from 45 to 50 per cent of resin."

As to M. Dumas, nothing can be more appropriate than the sketch here given of him. On entering his class-room, he does not, like M. Thenard, plunge at once *in medias res*†, and without preface recapitulate the whole of his last lecture, but carefully examining most of the phials which contain the elements of his experiments, he sees that everything is in order, and then taking out the papers containing the heads of his discourse, he straightens them, glances his eye over them, and after pausing a moment commences his address. His conclusion of the last of one of his courses was highly characteristic: "*Mon intention n'a point été de décrire la pratique des arts, mais bien d'en éclairer la théorie. Ces détails scientifiques qui effarouchent les fabricans d'un certain âge, ne seront qu'un jeu pour leurs enfans, quand ils auront appris dans leurs collèges un peu plus de mathématiques et un peu moins de Latin; un peu plus de Chimie, et un peu moins de Grec!*" ["My intention has not been to describe the practice of the arts, but to elucidate clearly their theory. These scientific details, which now terrify the adult manufacturer, will be mere trifles to his children when they shall be taught at school—a little more mathematics and a little less Latin; a little more Chemistry, and a little less Greek!"] Everything he does has an air of earnestness and calmness.

* The sweet *principle* of oils.

† Over head and ears.

His appearance is much in his favour; and you see at once from his face that he is a man of sound judgment, cool research, and firmness of character; yet, at the same time, that his mind possesses a great deal of delicacy and refinement. In his style, he is quiet, unpretending, accurate, and minute: warming seldom with his subject, although convincing you continually that he is perfectly master of it. He is the most gentlemanlike neat-looking professor at the Sorbonne.

Leaving the Sorbonne to visit the College of France, Mr. North presents us with the following bit of forcible *chiaro-oscuro* and rich colour in the professor of general experimental physics,

AMPERE.

"The friend of Davy, and whilome one of the great natural philosophers of France, is selected for this sketch, not from the space he at present occupies in science, but for *la petite comédie que voici* *, and the amiable old age he exhibits. You see a venerable *octogénaire* †, of small stature, clad in a coat of grotesque cut, on which the marks of climacterical decay are as visible as upon the excellent old man who has borne it for a quarter of a century. He has parted with his teeth, his memory, and his elasticity of step, but he retains his *bonhommie*, his delightful mannerism, and ever and anon exhibits some flickerings of that enthusiasm in the cause of science with which he began life, and without which nothing is to be done. I dare not, however, meddle with the splendid fragments of that genius which so often startles you into the conviction that a great man is really addressing you, I have been present at several amusing little scenes enacted between himself and his pupils: and one or two are so illustrative of amusing simplicity and a not-to-be-superannuated good-nature, that I shall venture to try their effect at second-hand. On the very first day I went to hear him (it was an introductory lecture) he had so filled THE *Slate* with first and secondary branches of the goodly tree of science as to leave no room for more boughs, unless by topping the head, and abridging the undue growth of the original shoots. Space was wanted, and the remedy should have been at hand; but, lo! the sponge had disappeared, and could nowhere be found, though the class showed much *empressement* in seeking it. At last, with a look most comically solemn, the old gentleman drew out his cotton representative for a *foulard* ‡, and looking first at the slate and then at the *mouchoir* §, plainly could not make up his mind to sully its gaudy colours by exacting from it the office of the sponge. But while necessity and reluctance were contending for the mastery on his features, the sponge was picked up by one of the students, and eagerly presented to M. Ampère, whose delight and manner of expressing it were irresistibly comic. Seizing it between both his hands, as if to be sure that it was not the shadow of the *veritable* detergent, but the very substance that he held, he hastened to the door, and putting his head out, called to his assistant, *à la Molière*, in the happiest and most unconscious imitation of the *de Pourceaugnac accent*—'*Je l'ai trouvé;* c'est à dire, *on l'a trouvé*—il n'entend pas—(aside). Monsieur! . . . Ecoutez donc!' . . . Then, at the highest pitch of his voice, '*Monsieur!* ne vous donnez pas la peine de la chercher; je *l'ai* ici—*on vient de la ramasser!*' ['I have found it; that is to say, it is found. He doesn't hear me—(aside).—Monsieur! I say, Monsieur, don't trouble yourself about it; I have got it here—they've just picked it up!'] Then, quite regardless, and apparently unconscious of what the French journalists call '*une vive explosion d'hilarité* *' from the class, he resumed as if nothing had occurred. He had been lecturing on the polarization of light and heat, and had assumed a square ruler and a pasteboard almanac to represent a cylindrical ray and a transparent medium of transmission, when gradually warming with his subject, he began (as one is apt to do in lecturing) to describe parabolas with his ruler, one of which encountered the tumbler (which is here *d'usage*†), and broke the pieces of glass into his *eau sucrée*‡—(without *eau sucrée* nobody could get on with a lecture at the College de France or the Sorbonne, though law and physic lecture with unlubricated fauces). Out of this half-demolished glass, he was presently preparing to drink, when half-a-dozen voices at once called out—'*Monsieur Ampère!* eh! *Monsieur Ampère, qu'allez-vous donc faire?*' ['Monsieur Ampère, oh Monsieur Ampère, what are you going to do?'] but he, nothing heedful of these exclamations, raised the tumbler to his lips, and began to sip its now dangerous contents. In an instant one of the foremost in the class springs forward and seizes the old man's hand, another wrests the tumbler from his grasp. A scene! profound silence in the class! The venerable man looks at them ironically, 'Thank you, gentlemen!—very kind of you!—but you are giving yourselves unnecessary trouble; I took it for granted that my class understood the laws of gravitation:—with your permission, gentlemen, I will first drink my *eau sucrée*, which I want, and will then give you a hint which you appear to want.' He now drank without further molestation, and then drawing in a long breath.—'*Eh! comment, Messieurs, voulez vous qu'il est eu du danger!—ne savez-vous donc pas que le verre est plus pesant que l'eau,*'—['What, gentlemen! then you thought there was some danger! but arn't you aware that glass is heavier than water? And did you not observe how careful I was to drink the contents of the tumbler at a reasonable angle?'] Then, taking up the tumbler, he continued to incline it over the table till it was nearly horizontal, and so on, till the pieces of glass fell out, and the class laughed. 'Ah! si je l'avois bu *à cette angle là!—mais j'ai été plus adroit!!*"—['Ah! if I had drunk at this inclination!—but I was too knowing for that.'] Here (for it was at the end of his lecture that this little episode occurred) a bright-eyed damsel went up and asked some question respecting the course of rays of light through certain media, but whether old Ampère referred her to his heart, as we should have done, we could not hear. She coloured, however; her eyes seemed pleased with the interpretation given to her question, whatever it might have been, and they walked out together, a 'January and May,' separated only by the insecure partition of the pasteboard almanac which the elder of the months still kept in his hand."

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* The little farce which follows.

† An eighty-year old.

‡ A real Bandana.

§ Handkerchief.

These specimens have raised our anticipations to a high pitch. On the left bank of the Seine, and *nearer its stream*, there are many first-rate subjects which we trust are already sketched in, and safely deposited in the portfolio of the artist. It is for these that we shall eagerly seize and cut open succeeding Blackwoods; and we think that a large class of our readers will sympathize with us.

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* A loud burst of merriment.

† Customary.

‡ Sugar and water.

A POPULAR COURSE OF CHEMISTRY.

I.

INTRODUCTION.

IT may not be uninteresting to the general reader, and the juvenile student, to present a brief, general, and popular view, of the mysterious art of Alchymy; and thus exhibit the source from whence emanated the brilliant science of Chemistry; this paper will, therefore, be devoted to the subjects of the olden art, as introductory to those of the modern science. Enthusiasm for a favourite object not unfrequently estranges men from the path of patient investigation, and leaves far behind their better discretion: this is strongly exemplified in the case of those authors, who, in their zeal for very remote antiquity, pretend that alchymy is as ancient as the world; and that some of the personages spoken of in the earlier pages of the sacred writings, not only understood the art, but also practised it to a very great extent. Giving up this distant period, others ascribe the origin of the art to Greek ecclesiastics during the fifth and sixth centuries; and even assert that alchymical manuscripts of these dates are yet in existence; but suspicions may be very reasonably entertained that they are forgeries of a far later age. The precise origin of alchymy is involved in the darkest and deepest obscurity, which it is impossible to penetrate; but it would appear that the claims of the Arabians are stronger and better authenticated than those of the Greeks: an Arabian writer of the seventh century, named Geber, is generally admitted to have been the first well-authenticated writer on, and practiser of, alchymy. The name of the art, and many of the terms employed in its practice, bespeak an Arabic origin: it would appear that the word ALCHYMY is compounded of the Arabic particle *Al*, signifying *the*, and *Kema*, *dark* or *secret*. *Alkema*, therefore, meaning *The Dark Art*, or *The Secret Art*, its earliest disciples were called *Alchymists*, and afterwards *Adepts*.

The writings of Geber attempted to prove that all metals are composed of mercury and sulphur; and that, by altering the proportions of these two substances in any of the baser metals, such as tin or lead, they would assume the properties of the nobler metals, gold and silver.

This change was usually styled *Transmutation*; and it was sought to be effected through the agency of a substance (spoken of for the first time in the writings of Geber), called *The Philosophers' Stone*, a fragment of which "projected" upon any base metal in a state of fusion, would cleanse it from its impurities, and transmute it into either gold or silver, according to the will of the operator. The principles of this alluring art, "like the refractory spirits of Arabian romance," soon escaped from the bondage of those who had first conjured them into existence; and quitting the Eastern clime they were introduced into Europe, probably on the return of the Crusaders. Spain first swallowed the golden bait; her example was followed with incredible rapidity by Germany, Italy, France, and England; and alchymy was cultivated with a zeal bordering upon madness, from the eleventh to the sixteenth century. Hundreds of volumes made their appearance in *these centuries*, relating to the art of alchymy; but it must not be imagined that these writings were couched in ordinary language, or in such plain terms that every one might read and profit by them; for, on the contrary, the writers deemed their secrets far too valuable to be exposed so openly, and therefore invented a curious style of language, replete with enigma, anagram, and mysterious symbol, which none but the initiated could decipher; novices who wished to penetrate the arcana of the dark art, were bound by an awful promise to abstain from revealing its mysteries, or even from holding converse touching them, excepting with those who could produce the requisite sign or counter-sign: and thus, although the secrets of alchymy were known to many, yet the adepts all held good fellowship.

It may, perhaps, be worth while to present a specimen or two of the style of these writings and their interpretation.

In discoursing concerning the metals,—a class of bodies particularly tortured by the alchymists; they use the terms Sol, Luna, Mercury, Venus, Mars, Jupiter, and Saturn, meaning gold, silver, quicksilver, copper, iron, tin, and lead; these seven metals were those chiefly known to them, and were imagined to possess some mystical relation to the seven planetary bodies both in aspect and properties. They also denoted them by the following symbols:—⊙ Gold; ☽ Silver; ☿ Mercury; ♀ Copper; ♂ Iron; ♃ Tin; ♄ Lead.

Besides these names and symbols, they were known by a variety of others; thus, Gold is sometimes called "the Sun terrestrial," "the King," "the Lyon;" Silver, "the Queen," "the Wife of Gold;" Mercury, "the Mighty Childe and Sonne of Gold and Silver," "the Eagle," &c.

The alchymist in appending his name to his writings, almost invariably concealed it in an anagram; thus, for example, "ANGELUS DOCE MIHI JUS*," will, if the letters are properly arranged, present the name of a far-famed alchymist, MICHAEL SENDIVOGIUS. Another writer appends to his writings the words "TWICE FIVE HUNDRED," which simply mean the initial letters of his name W. C.; the old way of printing the letter W, being thus VV, therefore VVC is twice five hundred.

The following is a specimen of their enigmatical writing:—"Hide and couple in a transparent den, the eagle and the lyon, shut the door close, so that their breath go not out, and strange air enter not in. The eagle at their meeting will tear in pieces and devour the lyon, and then be taken with sleep." The interpretation of this is probably as follows:—Put together in a glass vessel, quicksilver and gold, close the mouth of the vessel accurately, by melting the glass, to prevent the vapour of the quicksilver from escaping, or being mixed with common air. The quicksilver will speedily soften it, and losing its fluidity, will form an amalgam.

Here is another specimen:—"Take the most ravenous grey wolf, which, by reason of his name, is subject to valorous Mars; but which, by the genesis of his nativity is the son of old Saturn. He is very hungry, cast unto him the king's body that he may be nourished by it, and when he hath devoured the king, make a great fire, into which cast the wolf that he be quite burned, then will the king be at liberty again." This curious sentence may be interpreted thus:—

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* *Angel teach me right.*

Take common gray antimony, which may be reduced to the metallic state by means of iron, but which is closely allied to the nature of lead, melt it with gold, which it will purify; upon the continuation of a strong fire, the antimony will be dissipated, or form scoriæ with the base metals, and leave the gold in a pure state.

One more example of their curious writing, calculated to mystify the uninitiated, is—"Visitabis, Interiora Terræ Rectificando, Invenies Occultum Lapidem Veram Medicinam*." Now this would lead a stranger to suppose that it related to the philosophers' stone, or to some valuable medicine, whereas it relates to no such matter; the initial letters of the words are all that are to be attended to, and they form the word VITRIOLVM. So that this sentence simply relates to a single well-known substance,—VITRIOL.

The books on alchymy are very difficult to understand unless much time is devoted to their study. They abound in obscure directions for performing transmutation, long arguments in favour of its possibility, and testimonials of those who had either been performers or witnesses of the golden work. It deserves mention here, that there were two classes, who passed under the title of alchymists,—the genuine alchymist, who firmly believed in the truth and possibility of his art, and unhesitatingly devoted his entire life and substance to its pursuit; and the pseudo-alchymist, who believed in no such matter as the philosophers' stone or metallic transmutation, but used them as a means of existence, by practising upon the credulity of the ignorant wealthy: these designing personages were denounced with every epithet of contempt and scorn by the real alchymist. Though at the present day we may smile at the exalted notions of the latter, we should reflect that frequently the manifold wonders of their experiments took them by such sudden surprise, that calm and dispassionate reasoning was next to impossible. Everything was new to them; they were the first adventurers in an unexplored mine, and though without data to guide their progress, they struggled on, and opened galleries into the interior, which have been highly useful to their successors. We, in the pride of our knowledge, and facility of operating and reasoning, are but too apt to forget how much we are indebted to the long and painful labours of this very remarkable and despised class of men.

The alchymists frequently imposed upon themselves, by putting small grains of gold amongst their materials, "that like seeds they might increase and multiply;" and when this metal suddenly and unexpectedly re-appeared in an advanced state of the process, singular as it may seem, it was looked upon as resulting from transmutation.

When taunted by the sceptic to exhibit their skill, they sometimes adopted ingenious but dishonourable methods of imposing upon their challengers. They would conceal gold in false-bottomed crucibles, or resort to some processes of metallic precipitation, which served to convince the uninitiated that transmutation had actually been effected.

There was a remarkable circumstance almost invariably attendant upon the alchymist, which, to those who were not blinded by their ignorance or avarice, went far towards proving the fallacy of his art of transmutation, namely, extreme poverty; for although he strenuously asserted himself to be the possessor of the philosophers' stone, capable of turning lead into gold, yet he as constantly importuned the wealthy for the advancement of money to enable him to carry on his researches; and, strange as it may appear, numbers embarked and wrecked splendid fortunes in the work of transmutation.

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* *Visit the interior* of the earth, rectifying, thou shalt find the hid stone and true *medicine*.

To such an extent had the rage for alchymy possessed the minds of all classes of society, and, as an old author quaintly observes, "the estates of heirs and heiresses wasted so fast," in the reign of Henry the Fourth, that an act of parliament was passed, prohibiting the practice of alchymy: but this act was seized upon with avidity by the genuine alchymist, and held triumphantly forward as a convincing proof of the truth of transmutation.

In the reign of Henry the Sixth, things were so much changed, that this monarch issued four letters-patent for encouraging alchymy, and dispensing with all statutes and prohibitions to the contrary. Allured by the hope of some sort of gain, either of place, purse, or power, false alchymists now sprang up in crowds. Wider than ever now spread the notion of making gold by art. Royalty encouraged it; and removed all pains and penalties that had been attached to its practice: the consequence was, that these numberless impostors went about, pretending that they were in the possession of the philosophers' stone, and offering to communicate the secret of making it for a suitable reward. The amount of persons who were found credulous enough to be duped by such palpable impostures was really astonishing.

The very circumstance of claiming a reward for the disclosure ought to have been a convincing proof that there was no golden fleece to dispose of; for it is absurd to suppose that a man would stipulate for a pecuniary reward to divulge that, by the agency of which he could create tons of gold at pleasure. These adventurers persuaded their dupes to purchase enormous quantities of lead, in order, as they said, to "have stuff enough to project upon;" the surreptitious sale of which enabled the needy artist to obtain gold, which at a convenient season he melted in his crucibles, and exhibited it to the delighted eyes of his patron as the result of projection. Disdaining to mingle with this crew of impostors, the true alchymist still pursued his labours, amidst the obloquy with which the pretenders had covered his art. In defiance, however, of his painful watchings over the flaming furnace, and the glowing crucible, gold did not become more abundant with him: pale, wan, and care-worn, he was brought to the verge of the grave by incessant privation and toil, and frequently sank in the most abject poverty, in pursuit of the golden phantom.

Conjointly with transmutation, two other objects were sought after, namely, the universal medicine, and the universal solvent; the circumstances which produced the notion of the first of these, appear to have been as follows. Gold was deemed an indestructible metal; and it was therefore imagined that preparations of it being taken as medicine, might confer the same property of indestructibility upon the human body, and give immortality to the alchymist. The search after this Elixir of Life was, therefore, undertaken amidst the deadly exhalations of the laboratory, and carried on with much ardour and impetuosity: but though medicine was much benefited by the discovery and introduction of many valuable remedies during the search, yet diseases and death made no distinction between the profound adept and the meanest of mankind.

We are at a loss to divine the intended use of the universal solvent, or *alkahest;* nor do the alchymists condescend to inform us much about its properties, save that it was to reduce to the liquid state all substances with which it came in contact. This appears, at first sight, a preposterous inquiry, for it may be asked, in what kind of vessel could a substance capable of dissolving all things be contained? Modern chemistry, however, has succeeded in eliciting an intensely active substance, capable of dissolving all things excepting platinum. Now, it is not unlikely that during some of their curious and long-protracted processes they accidentally hit upon this intense solvent agent; and finding it incapable of being confined by ordinary vessels, platinum not being known to them, they at once styled it the universal solvent, and imagined it to possess extraordinary powers.

Such, then, is a brief and very general account of the progress of the art of alchymy, and its three grand objects,—Transmutation, the Elixir of Life, and the Alkahest. Ages were devoted to their pursuit, and innumerable volumes written concerning them. In these, may be found many most curious facts and observations, which prove that the alchymists were a class of men of far greater talent and abilities than we have been generally willing to admit. "The profoundest views of genius are everywhere to be found in their writings, allied with the most extravagant ideas."

Alchymy rapidly declined towards the close of the sixteenth century, and during the seventeenth its destruction was nearly complete. A few only of the most enthusiastic adepts then haunted its romantic ruins, and indulged in its golden dreams. These were at length dispelled by the gradual emergence of the science of chemistry. Like the fabled phœnix of old, it sprang from the ashes of its parent; and rapidly soaring amidst the most elevated sciences, proudly asserted its excellence and importance.

Chemistry cannot be said to have existed as a science before the seventeenth century; for although we find in the writings of the later alchymists, many curious discoveries, these remained useless and unapplied so long as the minds of men were occupied with the three grand desiderata. During this century, many philosophers of sound judgment were induced to examine the ruins of alchymy, and endeavour to place some of its materials to useful account. Divested of sordid notions, and anxious only to enlarge the boundaries of natural knowledge, they examined with much labour and skill such processes as appeared likely to offer useful results; classing operations and facts systematically together, and explaining them all in plainer language. Some of the more important compounds were introduced into medical practice by eminent physicians, with great success; others were applied in the arts and manufactures with a similar result. A host of philosophers entered upon this interesting field of inquiry, and the results of their united investigations soon conferred on chemistry the form of a science.

At the present day, experimental chemistry has attained vast extent and magnificence. The pages of its annals are adorned with names which demand our reverence and gratitude: "it has become a necessary branch of every liberal education," and has certainly "contributed as great a share towards increasing the resources, or adding to the welfare of mankind, as all the other sciences united." Chemistry now imprisons not its votary in the close laboratory to toil after the golden phantom of transmutation, the enviable elixir of immortality, or the mysterious universal solvent, commands him not to calcine and mix his medicines under certain planetary signs or influences, far less to distil the human skull, under the idea that as it is the reservoir of the most subtile spirits of the body, it must of a verity yield many good remedies for its diseases; nor does it insult his understanding by inculcating the fanciful notion that bodies preserve their figure, in virtue of a complex series of hooks and crotchets, of points and interstices.

The scene is gloriously changed! Emancipated from the spells of alchymy, astrology, witchcraft, and superstition, Science seeks now the society of Truth; and, leading her votary from the dark and secret cell into the bright sunlight of philosophy, she unfolds the wondrous book of nature to his gaze, and bids him become her "servant and interpreter." In fulfilment of this command he resorts to Experiment as the only true index of contents to the mighty volume. However laborious such a proceeding may be, however uncongenial to the aspiring mind that would fain soar amidst the seductive regions of theory, yet true sterling knowledge can never be gained without experiment. Chemistry may with correctness be called the universal art, for no production of nature escapes its operation. By its aid the structure of the globe is examined, and substances are extracted of paramount utility in the arts of life. From the alpine granite, the mountain marble, the crumbling sandstone, nay, from the very dust beneath our feet.

Chemistry directs attention to the beautiful phenomena of animated nature, traces the curious and intricate changes which a seed undergoes during its germination, and its growth into a perfect plant. In its fallen leaves, which, to the careless and ignorant observer, appear dead and lost for ever, it discovers living germs, which may again become active in producing living and luxuriant foliage. It also teaches us the curious fact, that although the varieties presented by the vegetable world are so widely different in external structure and character, yet they are all, with very few exceptions, constituted of the same elements, which are but few in number, and whose proportions are but very slightly varied. It matters not whether we subject to examination the oak, the pine, the willow, the lily, the lichen, or any other product of the vegetable world, each will yield up only three, or at the utmost four constituents, the further developement of which baffles the utmost skill and ingenuity of the experimenter. The continuous support of vegetable and animal life, chemistry proves to be dependent upon the atmosphere which surrounds the earth; and that the withdrawal of this aliment to any extent, impairs the powers of vitality.

The wonderful skeleton by which the animal body is supported, is ascertained to owe its strength and firmness to the presence of one of the gaseous *elements of this atmosphere*, in union with a highly inflammable metal, and a non-metallic substance barely inferior in inflammability. These three distinct and opposite bodies are, by some recondite and inexplicable operation of nature, absorbed singly into the animal system, and accreted into hard and compact bone; chemistry teaches yet further, that the same constituents which are found in all the varieties of the vegetable kingdom, compose all the softer solids of the animal frame, though combined in other proportions.

In the recesses of the gloomy mine, when threatened with instant and complete destruction, chemistry teaches man to protect his flaming taper with a thin metallic shield; and thus armed, fearlessly to continue his researches. By this secure and warning light he obtains his mineral fuel, whose combustion enables him to warm his dwelling, and to disengage a useful metal from a stone, which the artisan fashions for the purposes of the philosopher, the agriculturist, and the warrior.

Chemistry teaches that this valuable mineral fuel contains an aëriform principle, admitting of extraction in an insulated form, and eminently combustible and luminous. It teaches to operate upon it in suitable apparatus, to store it up in vessels of vast magnitude, and ultimately to transmit it through tubes hundreds of miles in extent, in order to furnish a means of brilliant illumination, during those hours in which the sun is sunk far below the horizon.

Chemistry examines the cause of dew, rain, hail, and snow, expounds the active parts which they enact in the vast laboratory of nature, instructs man how to reason calmly upon the awful and astounding phenomena of the volcano, the tempest, and the thunder-storm; and like a mighty enchantress, places in his hands a talismanic rod, whose metallic point, elevated towards the thunder-cloud, quickly disarms it of its destructive fire, and conducts it, innocuous, to the humid recesses of the earth.

The mere contact of two metals gives birth to one of the most astonishing agents of chemistry. Gifted with intense and almost incredible energies, it resolves the most refractory compounds into their elementary constituents, and entirely suspends the usual course of attractions. From the ashes of a plant it emancipates a metal, which floats and burns upon water, and inflames on the contact of ice. The same powerful agent, when modified in its intensity and properly conducted through an animal body, newly deprived of life, excites its powerless fibres into activity, and enables the experimenter to make a fearful approach towards the mysterious and incomprehensible principle of vitality. The dark and ponderous loadstone, which for ages has been exclusively employed for guiding vessels safely from clime to clime, chemistry has compelled to serve another purpose, and to elicit sparks of brilliant light at the will of the experimenter. The limpid spring, the babbling brook, the gushing fountain, the mountain-torrent, all yield up their secrets to chemical research. Their peculiarities are all examined, and found to depend on some impregnation, metallic or sulphureous, earthy or gaseous.

The wide waste of waters also offers "ample room and verge enough" for extending the dominion of chemistry; the saline taste of the ocean's waves is too remarkable to pass unheeded, and the cause of this peculiarity is found to depend upon the presence of several curious compounds, all of which may be extracted, and applied to useful purposes, in medicine, arts, manufactures, and domestic economy.

Shells, corals, and other marine productions, are all examined, and their composition ascertained; the animated beings which inhabit the vast and wondrous deep, are discovered to breathe atmospheric air, dissolved in the water so as to suit their peculiar organization. Nor does chemistry pause here in its researches; it discovers that the waters of the gushing spring, or the briny ocean, when freed from earthy and saline impurities, may be changed from the liquid state, and resolved into an explosive mixture of gases. It shows that these, when artfully burned together, produce the most intense heat; and their compound flame is applied successfully in metallurgy and other arts. When urged upon the earth of marble, it evolves "intolerable" light, rivalling that of the sun in the power and purity of its beams; which, directed through proper lenses, enables the naturalist to carry on his researches amongst the atoms of the creation. The sparkling diamond that we prize so highly, chemistry demonstrates to be merely charcoal in a crystalline form; and that the valuable gem may be readily resolved by fire, into those aëriform matters which constitute alike the noxious exhalation of the Grotto del Cane, and impart a grateful taste to Champagne and Moselle.

Chemistry also teaches man that the sand on the sea-shore, when mixed with the calcined ashes of a sea-weed, and exposed to the intense heat of the furnace-fire, quits its pulverulent form, and assumes that of a transparent solid, of astonishing beauty and utility. It is employed by the astronomer to form lenses of prodigious power, by which, properly adjusted, he is enabled to watch the motions and penetrate the abysses of the starry heavens: without this interesting substance, many of the sublime truths of astronomy,—the researches concerning heat and light,—the natures of numberless aëriform bodies, would have remained unknown, nor could "the naturalist have investigated the forms and appearances of that wonderful part of the creation whose extreme minuteness eludes the observation of the unassisted eye." In place of the clumsy wooden cups and uncouth vessels of coarse earthenware, employed in domestic use by our forefathers, we range upon our tables crystal drinking-vessels, of the most elegant design and skilful workmanship; and spreading the same material into sheets of great size and regularity, we set it in the apertures of our dwellings, to exclude the rude winds, but to permit the cheering rays of light to enter; a luxury unknown even to the rich in former ages, but now enjoyed by every class, the most indigent not excepted. To a similar process we are indebted for all those beautiful and elegant services of porcelain which adorn our tables; all the brilliant and lovely colours with which it is tinted, are the products of the chemical laboratory, where they have been extracted from metallic bodies, by the torturing agency of acids and fire.

Chemistry presides over the production and permanence of all those beautifully vivid and varied colours which tint the fabrics of the loom, and points out to the manufacturer the method of simultaneously creating many together upon his goods, or of instantaneously destroying all, and *leaving the surface of an* exquisite whiteness. Our modern habits and *fashions have rendered these* arts of paramount importance; and, in conjunction with others, they contribute not only to the gorgeous splendour of the princely court, but to the neatness and decoration of the peasant's cottage.

Whatever researches are undertaken by the chemist, his grandest results are utility, and the application of scientific principles to the arts and manufactures. "The true end of all science is to enrich human life with useful arts and inventions." These are the words of the illustrious Lord Bacon; and they are worthy of record in letters of gold, in the laboratory of every experimenter. The vast and rapid advances which have of late years been made, not only in chemistry, but throughout the entire circle of science, may safely be referred to the steady pursuance of the plan of "inductive reasoning" laid down by this philosopher, in his *Novum Organon*. Previous to the appearance of this singularly acute and profound work, the state of philosophy was deplorable; and he who could frame the most fanciful theory, and incomprehensible hypothesis, or mystify his doctrines to the highest degree, appears to have been regarded with the greatest veneration. Truth was either entirely sacrificed at the shrine of self-aggrandizement, or her fair proportions so distorted and mutilated, as to be almost undiscoverable. Experiment (saving alchymical experiment) was scarcely ever resorted to; indeed, the philosophers of that day deemed its practice far beneath their dignity; and held in sovereign contempt those who were the advocates for, or "makers of experiments," styling them "a tribe of idle curious people, whose philosophy consisted in making experiments on the gravity of air, the equilibrium of fluids, the magnet," &c. "A philosopher," said they, "should go no further than the *contemplation* of things, leaving the *execution* thereof to another set of men; though he should have a certain *theory* thereof, in order to judge *pertinently* of them."

The true philosopher, however, at length discovered that the contemplation of things was not incompatible with practical execution, and that the "lover of wisdom" could suffer no real degradation in the eyes of his fellow-men, by resorting to manual labour, in order to verify the workings of his mind. The splendid series of discoveries that has been consequently made in all departments of knowledge, but especially in chemistry, has enabled man to work wonders upon matter, in all the various forms of it with which he is surrounded, causing them each in turn to minister to his wants or his luxuries, and thus triumphantly to assert the truth of the Baconian precept, that "Experiment is the basis of philosophy," and that such "knowledge is," indeed, "power."

The safety-lamp and the steam-engine are magnificent examples of the application of refined principles of science to purposes of practical utility; and they constitute two of the brightest gems in the diadem of experimental philosophy. Nothing is now considered beneath the notice of the true experimentalist; and facts or observations the most simple, have given birth to discoveries and results of the greatest magnitude and importance.

In chemistry this is perpetually occurring; and with the view of showing how intimately its admirable laws and important principles are connected with arts and manufactures, and with various operations of constant occurrence in domestic life, it is intended that the present introductory paper shall be followed by a series, giving a familiar but a connected and strictly accurate view of the science and its applications.

THE GALLERY OF PRACTICAL SCIENCE.

No. II.

1. Manby's Lecture on saving Lives from Wrecks.
2. Automatic Ship and Sea.
3. Magnetic Needle.

Manby's Lecture; Wednesday, 24th February, 2 p.m.

Captain Manby delivered, in the Long Room of the Gallery, a lecture on the most efficacious means of saving the lives of shipwrecked sailors, and illustrated his description by numerous models of the various apparatus invented by him; and which is happily, now, very extensively and successfully used in many instances on our own shores, and in several other parts of the world.

This veteran in the cause of humanity, after an application of nine and twenty years to the prosecution of his philanthropic labours, seems to have lost little of his energy. He gave, in a condensed form, the results of his long experience, and it appears that he can look back upon his past life and claim the singular and honourable distinction of having been the means of saving the lives of six hundred of his countrymen, and of one hundred of foreigners! *Has he yet received* ONE *civic crown?* He has latterly been pursuing his favourite object, that of rescuing his fellow-creatures from painful and horrible death, in other directions, and has now ready to present to the world, plans for the saving of drowning persons under whom ice may have broken; for the prevention of fires in buildings, and for the rescue of persons from houses, &c. in flames.

The profits which may arise from his publication, he proposes to devote to the construction of a complete apparatus, designed by him, for the assistance of persons immersed in water, in consequence of the giving way of ice beneath them, and which he is anxious may be ready before the ensuing winter, and thus prevent the possibility of the recurrence of the distressing events which happened on the Serpentine River last Christmas-day. He also proposes to subscribe five pounds in addition.

The Captain thought it his duty to make a statement, which, we submit, would become the Humane Society to notice, in order to remove even the shadow of a suspicion of any misapplication of the sacred funds intrusted to their charge. For ourselves we disclaim having the slightest, and are perfectly satisfied that there can be no real ground for any; but it is enough that some persons think otherwise, and that the statement has been publicly made. It is, that the late Dr. Fothergill left five hundred pounds to be applied by the Humane Society to the "prevention of shipwreck," and to "the preservation of shipwrecked mariners;" and that although the estimable devisor had been dead many years, not a solitary application of money to this object had yet been made by the *Humane Society*.

Automatic Ship and Sea.

In our last Number we gave an account, in the "Standard Clock," of a combination of exquisite and carefully-constructed machinery, designed to attain an object of the highest importance, and certainly one of the most difficult accomplishment. The utility of such a machine may be estimated in a greater or less degree by every one, but its peculiar merits and construction can only be appreciated correctly by the highly-scientific few. In the present article we propose to address a much more numerous class than the latter, in which we particularly include our juvenile readers; we offer a subject, actuated by the same kind of agency, but having no pretension beyond that of being an agreeable object of observation, and which we intend to make instructive, by delineating and explaining the machinery through which its peculiar effects are produced.

The object we have selected for this purpose is one lately added to the collection in the Gallery—A SHIP AT SEA. To those of our readers who may not have seen this automaton*, we must premise that it is one of the most successful attempts at imitative motion ever accomplished. It is perfectly free from all those interrupted *staccato* effects which generally mar the finest productions in clock-work; and it most faithfully exhibits the easy, ever-varying, and ever-blending changes of position and surface, which a steady stiff breeze will produce on a flowing sea, and a vessel under full sail. It is surprising to see how accurately two of the most magnificent instances of nature and art are embodied, and their peculiar movements enacted on so small a stage—a field of ocean heaving with life, and a man-of-war floating, sailing, and even vibrating with the roll of the waves beneath her; enclosed by a glass-guard, and an oval of a few hands area.

The sympathy, if we may so term it, of the ship with the sea, is admirable; when she seems to overtake a wave, her bow slides up its side, and is projected into the air; as she rides on its breast, her stern also becomes elevated, and her deck is, for an instant, horizontal; and then, as she leaves it, her bow is depressed, and she sinks bodily down into the succeeding hollow. This last effect is so perfect, that a lady, visiting the gallery, was heard to exclaim to her companion, "*Do come away; that subsidence is really so natural, that it brings all my recollections of sea-sickness about me.*"

To give an idea of the actual size of our vessel, we may state that, from stem to stern, she measures five inches and a half, so that she appears to be not much larger than her portrait in the annexed diagram.

Though the effects are so perfect, yet the mechanism, it will be evident, is very simple. It is concealed in the model from the observer, by a membrane (*v*), which is attached to the hull, and thence extending to the borders of the machinery-chest, is there fastened. This membrane is very delicate in its texture, and extremely pliant; it is not strained tight, but, on the contrary, left very full; and its surface is painted to represent an agitated sea. In all the elevations and depressions of the vessel, this

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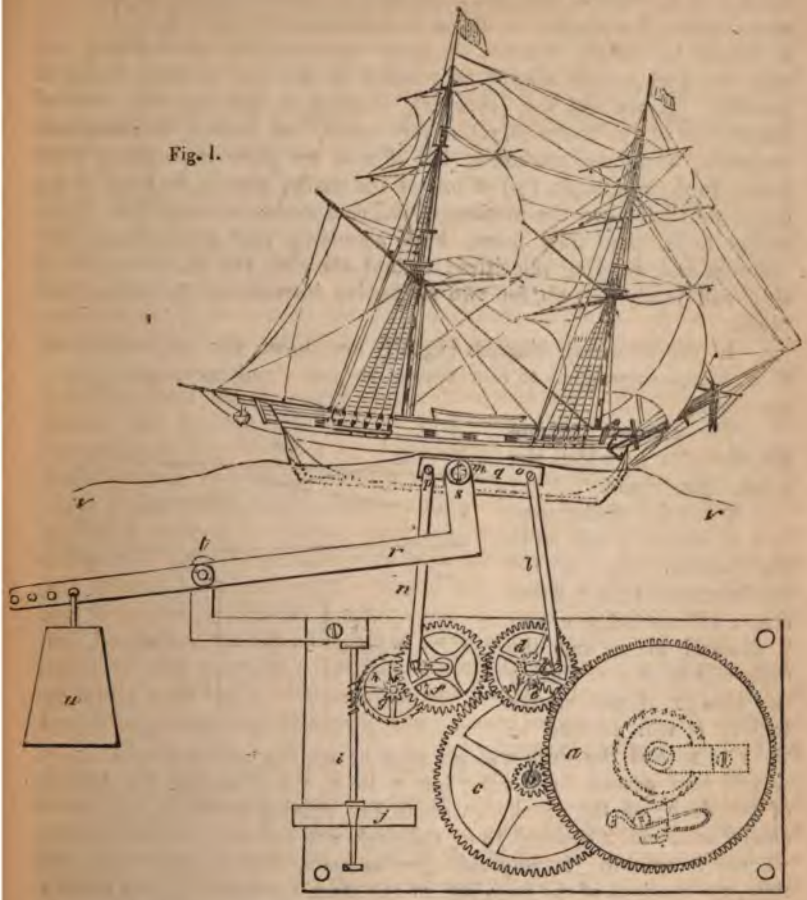
* A machine that has the power of motion within itself.

Fig. 1.



membrane of course accompanies it; but to the spectator, the motions of the vessel seem to be the effect, and not the cause, of the waves.

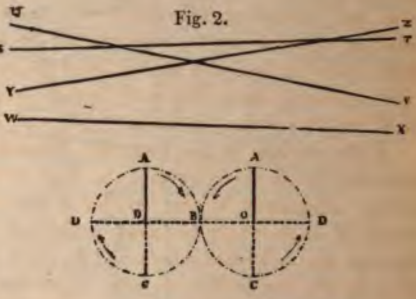
In the diagram (fig. 1), one of the containing plates of the machinery is removed, to show the connexion of the parts. A spring contained in a barrel (*a*), communicates motion through a train of pinions and wheels (*b*, *c*, *d*), to two wheels (*e*, *f*), which have each the same number of teeth, and are geared together; on the axis of these wheels are cranks (*m*, *k*), which move two shafts (*l*, *n*), attached by centre-pins (*o*, *p*,) to the keel (*q*) of the vessel. To this keel is also attached, by a centre-pin (*s*), a lever (*r*), which, resting on a fulcrum (*t*), is continued beyond to any convenient length, and has, near its end, a moveable weight attached (*u*). One of the cranked wheels (*f*) is geared by a pinion and wheel (*g*, *h*) and an endless screw (*i*), with a fly (*j*), for regulating the velocity.

Supposing *the lever* (*r*) *to* be removed, the cranks and the shafts

($m, k,$) ($l, n,$) vertical, and the machinery in action; it will be seen, by examination, that motion would be communicated to the vessel, but that it would be simply vertical, a mere up-and-down movement, and that the deck would always be parallel to the line in which it lay at starting. If we add the lever (r), centring it midway between the centre-pins of the shafts ($o, p,$), a very small, but scarcely a perceptible variation, would be produced; but if now we place its centre-pin (s) nearer to the centre-pin (p) of one of the shafts, than to that (o) of the other, we shall have the motions of the centre-pins so controlled by the radius ($s\ t$), that they move, both ascending and descending, with different and differing velocities; so that the stem and the stern of the ship will rarely remain for two successive instants in the same level plane.

In the following diagram (fig. 2), are shown the positions of the deck, which correspond to four successive and simultaneous positions of the cranks.

Fig. 2.



The arrrows indicate the direction in which the cranks turn round.

When the cranks stand at O A, the deck will be in the position S T; as the cranks move to the position O B, S will ascend to U, and T descend to V, and the deck will be at U V; during the change of the cranks to O C, U will descend to W, V to X, and the deck will attain W X; let the cranks go on to O D, W will now ascend to Y only, but X to Z, Y Z becoming the position of the deck; as the cranks go on to the starting positions O A, Y will ascend to S, and Z descend to T, the deck will arrive at S T, the position whence it set out. It may therefore be seen, that in each interval of time, the motions of the stem and of the stern are different, one of them being always greater than the other, and that at two points in the course, the one which was the greater becomes the lesser, and *vice versâ*. It is owing to the ingenious introduction of the lever (r) into its peculiar position, with regard to the shaft centre-pins ($s, p,$) that this play of changes takes place, and the pitching of a ship in a brisk gale and high-running sea, is so beautifully imitated. By the weight (u) this pitching can be made quicker or slower, at pleasure.

The invention is French, and patented. The names of *T. C. Cailly* and *Eude*, are stamped upon the machinery-case.

Magnetic Needle; Monday, 29th February.

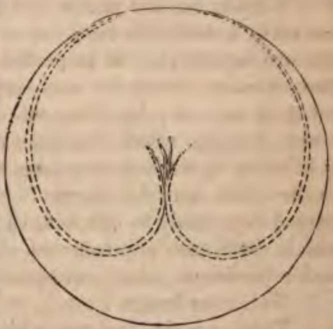
The council of the Society directed that a very large magnetic needle should be immediately constructed, and fitted up in the most careful manner, by Mr. Saxton; and be deposited in the Gallery, for the purpose of effectively showing the variation, oscillation, &c., of the magnetic needle, at London.

REVIEW.

I. *Optical Investigations.* 1. *Caustics.* 2. *Optical Images.* By the Rev. G. H. S. Johnson, M.A., Tutor of Queen's College, Oxford. Published by Talboys, for the Mathematical Society.

We congratulate the University of Oxford on the institution of a Mathematical Society. This body, though we are informed it is extremely limited in point of numbers, is yet, we conceive, likely to be of great importance in furthering and encouraging mathematical studies, so little *generally* pursued in that university, yet, as we have now abundant evidence, carried to so high a point by some few distinguished individual members. If it do nothing more than act as a printing-machine for memoirs like those now before us, it will do much; but we trust that its meetings may yet be productive of more extensive good. This society has as yet published only the two papers above-named, which, though printed as separate tracts, may in fact be regarded, in connexion with a previous publication of a similar kind by the same author (relating chiefly to observations), as a series of investigations on some of the most important topics in what is distinctively called mathematical optics.

With regard to the first, the subject of caustics is one which we cannot say much to illustrate, in the compass of such a notice as the present. A caustic is the luminous line or curve formed by the intersections of rays of light after reflection, at curved surfaces, or after refraction through media bounded by such surfaces. A familiar example is seen in the inside of a tea-cup, when the rays of a candle shine into it. On the surface of the liquid there will be seen a curved line of light, whose form, relatively to the circular outline of the cup, is represented in the annexed sketch.



Such curves, formed under different circumstances, are an elegant subject for geometrical investigation; and have been treated by Mr. Johnson, in the tract before us, in a very original and beautiful manner, accompanied by some illustrative examples, which we believe are new.

The subject of the second tract is one referring to matters more familiar to every one who has seen optical experiments, and of more general interest; and on which it may be worth while to say a few words, to remove an ambiguity into which some writers fall. An optical image may be defined to be the formation of a distinct point of light for every point in the object whence a ray of light originates, and in the same relative position; so that these points of light, collectively, (if their positions were marked,) would give rise to a picture or resemblance of the object, either exactly similar, or distorted in some of its proportions, according to circumstances.

Such a formation of corresponding luminous points may take place in several different ways; and the position of those points, and consequent depicting of the image, may be produced in several ways; but the essence of an optical image is their actual formation by whatever means.

Thus, in the simplest case of the camera-obscura, the hole in the shutter and picture of external objects on the wall: from every part of the object (suppose the flame of a candle), a body of rays reach the hole. The minute aperture only allows a single narrow ray to pass; this reaches the wall or skreen, and, being thus limited and defined, and of sufficient intensity (since it is contrasted with the darkness of the room), will give (at whatever part of its course it may be stopped) a luminous point: all the other rays do the same in their respective directions; hence, at *any distance*, an image is painted on a screen.

Next, let us take the case of a plane reflector. From the flame of a candle, as before, rays fall on every part of the mirror, and are reflected: but on no wall or screen is an image of the candle produced: The image is often said to be formed behind the mirror. Even suppose the silvering removed, a screen placed behind will have no image depicted on it. When, then, is it formed? or is there any image at all? We reply there is none in the sense commonly adopted; but there is one formed in the eye (as a camera-obscura,) by the precise process of the last paragraph (modified only by the introduction of a lens at the aperture). It is matter of easy investigation, however, to show that these rays which fall upon the eye, come in the same directions as they would have *if* they proceeded from a real object, situated at the same distance behind the mirror as the eye is before it.

The same thing precisely may be said of *convex reflectors.* The eye is deceived into the belief that an image exists behind the mirror; and, from the course of the rays, we may speak mathematically of an image existing there, but the whole is a geometrical fiction.

With a *concave* mirror it will be said the case is different: here we can really exhibit the image on a screen: it is actually formed in the air. The eye, again, may be so placed as to see it. But these are two totally distinct cases. They are each separately simple cases of optical images; but they are not related to each other: the image, as formed in the eye, is not an *image relatively* to the mirror. The rays come from the mirror *as* from an *object*, and they form an image in the eye as a camera-obscura. In the other case, the image is *really* one *relative to the mirror*, and is formed (agreeably to our definition) by the small definite pencils of rays which, from each point in the object, come by a separate course to give each a luminous point, corresponding to the point of its origin, and thus, collectively, an image.

This is, again, the case with lenses. The essential point, as before, is here the formation of a separate defined focal point of light for each luminous point in the object. When these are formed collectively, there is an image. In convex lenses it can be depicted on a screen; it can also be perceived by the eye. But the two cases are again essentially distinct; the one is an image belonging to the *lens;* the other to the camera-obscura of the *eye.*

Now the image formed in the eye, by the rays proceeding from a refracting surface, or through a reflecting medium, *as if* from an *object*, will be modified in a variety of ways, according to the course which those rays take; and the object of the mathematical inquirer is to investigate what directions will be given to rays, supposed, in the first instance, to come from a luminous point, and then to be reflected, or refracted, according to the known laws of reflection or refraction, at surfaces either plane or of any given geometrical species of curvature. According to these conditions they will seem to come, as it were, from an object of very different shape, and the determination of this constitutes what is termed the mathematical investigation of optical images. To this subject Mr. Johnson has directed his powerful mathematical abilities, and has treated it in a very simple and elegant, if not wholly original manner. It would, of course, be entirely unsuitable to our pages to enter upon details; but we will mention one result at which he arrives, which is somewhat singular. A straight line, or stick, wholly immersed in water, always appears to the eye sensibly *straight*, though its apparent position is changed. Mr. Johnson was conducted, by his mathematical analysis, to a certain algebraic formula, which ought to express the nature of the image in this case; and instead of a straight line, (which would, algebraically, be expressed by a simple equation,) he found a curve of a high and complex order. When, however, he proceeded to inquire more closely into the nature of this curve, he found that one part or branch of it only, was actually concerned in the problem: and that branch was found to have the remarkable property of taking a form so slightly curved that to the eye it would be sensibly confounded with a straight line: thus evincing the singular accordance of geometry with nature in the midst of *apparent* discordance.

II. *Perspective Rectified; or, The Principles and Application Demonstrated. With a New Method of Producing Correct Perspective Drawings without the Use of Vanishing Points.* By ARTHUR PARSEY, Professor of Miniature Painting and Perspective. 4to., 16 Plates. London, Longman and Co.

WE perfectly agree with Mr. Parsey in his opinions, both as to the advantages of a knowledge of perspective, and the facility of acquiring that knowledge by any one inclined to take the pains. Mr. Parsey has obviously thought much and justly on the principles of art: many of his observations are original and important. We are, therefore, surprised, that with his qualifications, he should have failed to see that *Linear Perspective* is strictly a branch of geometry, which can neither be learnt nor taught, but by a rigid adherence to mathematical deduction and demonstration. The fact, as we conjecture, is that the author has studied the subject more with the feeling of an artist than with that of a geometrician, for his train of reasoning and language are deficient in that precision and accuracy so essential to all mathematical investigations.

There is no question but that the apparent forms of objects, as impressed on the retina, are modified by the construction of the organs of vision; it is these forms that the artist endeavours to transfer to his canvas *when he draws by eye*, as it is termed, and his power of doing this with judgment and facility will doubtlessly be increased by a general acquaintance with the rules of linear perspective: but this last-named art waves all optical considerations, and only professes to furnish the means of delineating on a surface, the contours of figures, bounded by simple geometrical lines and surfaces, as they would be seen by an eye *considered as a geometrical point:* hence it is, that all delineations obtained on these principles are, in fact, incorrect, when viewed from any other than the precise point from which the outlines were deduced, and which is taken to represent the eye of the spectator; the geometrical draughtsman, however, aware of this, shows his judgment by selecting his point of view, so that no obvious distortion may be apparent in his outline, when viewed from any point indifferently as drawings usually are.

It is true that the parallel lines of the top and bottom of a long wall would always be projected on a *plane* into straight lines, but they are projected into *curves* on the *concave* retina of a person standing opposite the middle of the length of such a wall; and the lines in question do, to him, seem to approach each otheon either side of him, and yet present no angle, an appearance which certainly implies that they are seen as curves*.

A person of Mr. Parsey's talent and reflection might make a valuable addition to the literature of art, by a work on the subject to which we have referred; but this is not the object of the work before us, which professes to teach the geometrical art commonly termed linear perspective, and we must own that we regard it as a failure.

The system recommended by the author was that generally employed, before Dr. Brook Taylor and Mr. Hamilton, towards the close of the last century, placed linear perspective on its legitimate foundation, and gave it all the precision and elegance which distinguish it. We think, therefore, that Mr. Parsey, in endeavouring to simplify the apparent difficulties of the practical application of the principles established by these masters, has made an innovation without any improvement, and has really sacrificed both accuracy and intelligibility.

He seems to overlook the fact, that an outline on paper ought to be the section by a *plane* of the cones of rays proceeding from the contours of an object by the eye; and that by taking the chords of the angles subtended by the original lines, the outline he deduces is essentially false, because he thereby assumes several unconnected planes of projection. For example, in his fifth plate the object is a cube, and he assumes the plane as *not* parallel to a face of the solid; in this case the upright lines of the cube would have a vanishing point, that is, the lines representing them would not be parallel as he has drawn them; in fact the whole of his construction is totally erroneous in this and several other instances, though the style of the diagrams prevents the defect from being immediately apparent.

We would earnestly advise any one desirous of acquiring the principles of perspective, to have recourse at once to writers who have treated the subject purely mathematically, and they may be assured that they will find no difficulties if they are conversant with plane and solid geometry; without that previous knowledge, they will never master the subject.

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* *We will take an* opportunity of elucidating this, and some other points on the *subject, in a future* Number of this Journal.

III. *Annual Report of the Royal Astronomical Society of London*, dated *Feb.* 12, 1836. Printed for the Society.

An early copy of this Report having been kindly put into our hands, we gladly avail ourselves of the opportunity of presenting to our readers a portion of its interesting contents, though we are obliged to use some editorial privileges to make room for our notice. It is not, however, that we intend to enter into any account of the affairs of the Society, or to notice here the progress which the science it cultivates has made within the last year, gratifying as both are; it is the tribute of respect paid in the report to the memory of three of its most distinguished Fellows, whom it has lost during that period, which solicits our attention.

The debt of gratitude which society at large owes to such of its departed members who have promoted the welfare of their race, is sometimes discharged in part by the monument erected by the artist, and by the biography of the literary man; each helps to extend the knowledge of the benefits which the community has received from those who are no more. And as the desire of posthumous fame must ever be the strongest stimulant to the rightly ambitious,—to those who, by their talents, are capable of bettering the condition of their fellow-creatures; it behoves mankind, even from selfish considerations, diligently and faithfully to acquit itself of this obligation; we, on our parts, will not be backward in paying our quota.

The precise and formal nature of an official report, necessarily keeps down the expression of those feelings in which a writer would otherwise naturally indulge, when recording his recollections of a deceased associate; conscious that his memoir may be read by thousands who never saw, if they even ever heard of him whose character he is drawing, he thinks it necessary to restrict himself to a simple enunciation of the events, or of the labours and discoveries which have entitled his subject to celebrity. Hence arises, as we presume, the subdued tone of eulogy in the biographical sketches before us; but as we do not feel ourselves under such restrictions, we, while we avail ourselves of the substance and words of the memoir, shall unhesitatingly give expression, in one instance at least (the second in the following series), to that enthusiasm which we feel for a man not known as he deserved to be, whose moral virtues were even more rare than the professional genius and extensive knowledge, which placed him in the foremost ranks of his contemporaries.

Dr. Brinkley,

"Bishop of Cloyne, was for a long time Andrews Professor of Astronomy at Trinity College, Dublin, and Director of the Observatory near that city. Indeed, he had spent so much of the latter period of his life in Ireland, that he has been considered by some as a native of that country. He was, however, born in England, and of English parents. He distinguished himself in early life at the University of Cambridge, where he was the senior wrangler of 1788. He was for a short time an assistant at the Observatory of Greenwich, where probably he acquired the taste for astronomy which he afterwards cultivated with so much success: and it is to this circumstance also that he was indebted for the appointment of Director of the Observatory in Ireland. *For when Dr.* Maskelyne was requested to point out the most qualified person that he knew, as a successor to Usher, he instantly named Brinkley. The observatory at that time was furnished only with a transit; and this state of comparative leisure gave him opportunities of attaining greater proficiency in transcendental mathematics than any of his contemporaries, and at a much earlier period. Most of his papers on subjects of this nature are inserted in the *Memoirs of the Royal Irish Academy*, and are duly appreciated by those who are conversant with such inquiries. On the erection of the circle, he applied himself more assiduously to practical astronomy; and justly estimating the powers and advantages of such an instrument he devoted his time to the elucidations of certain minute subjects in astronomy, which had hitherto either evaded the researches of former observers, or had been the subject of much doubt and even controversy: such as the aberration and parallax of the fixed stars, the solar and lunar mutation, and the varied amount of astronomical refraction, more especially at low altitudes. In the investigation of these subjects we trace the same master-hand as in all his other inquiries: and although the result of his deductions relative to the subject of parallax does not appear to accord with those obtained from the mural circle at Greenwich, yet so highly did the Royal Society estimate the talent and skill displayed in the inquiry, that they awarded him the Copley medal for his paper on this subject. The constants of aberration and lunar mutation, determined by Dr. Brinkley, are those which have been adopted by this Society, in the formation of their catalogue of stars: the former deduced from 2633, and the latter from 1618, comparisons of various stars. This attempt to deduce from observation the constant of solar mutation (the existence of which was only known from theory) is at once a proof of his skill in the manipulation of the instrument, and of his confidence in its accuracy and its powers. His tables of refraction, when adapted to the external thermometer, are found to possess a degree of merit far above what has been generally attributed to them. In private life, Dr. Brinkley was remarkable for the kindness of his disposition, and the urbanity and mildness of his manners; and was ever ready to communicate information to the zealous and earnest inquirer after knowledge. On his promotion to the see of Cloyne, he devoted himself almost entirely to ecclesiastical affairs; and for the last ten years he had not contributed a paper to any scientific society. He was for a long time the President of the Royal Irish Academy; and for two years filled also the chair of this Society. He died on the 13th of September last, at an advanced age.

"The following particulars of Brinkley's earlier life have been communicated by a Fellow of the Society. Both he and Vince, when boys, were under the care of Mr. Tinley of Harleston, to whose assistance and influence over others both owed the means of maintaining themselves at the university. Brinkley wrote in the *Ladies' Diary* from 1780 or 1781 to 1785; and mention is made of his name by Maskelyne, in the Greenwich books, from June 1787 to March 1788, in which interval he also took his degree at Cambridge."

EDWARD TROUGHTON

"was born, it is believed*, in October, 1753, in the parish of Corney, in the county of Cumberland, the third son of a small farmer. An uncle of the same name, and his elder brother, John, were settled in London as mathematical instrument makers, and as his second brother was apprenticed to the same business, Edward was designed to be a farmer, and continued to be his father's assistant till the age of seventeen. To persons acquainted with the condition of that part of England, it need not be said that his education was better than is usual for his rank, and was of a sound though homely cast. There has always been there, so far as memory goes back, a considerable stock of mathematical and general knowledge floating amongst the people; and a large proportion of persons distinguished for their attainments in the university of Cambridge have arisen among the Cumberland and Westmoreland yeomanry. The death of his second brother altered Edward's destination, and he was immediately placed with his brother John, then chiefly employed in dividing and engraving for the trade and the higher branches of the art. Under his instruction, Troughton made most rapid progress, and, at the expiration of his time, was admitted as a partner. About 1782, the Troughtons established themselves in Fleet-street, where they commenced an independent business, as successors to a series of well-known artists (Wright, and subsequently Colex), who had previously occupied the same premises. Ramsden was then in the zenith of his reputation, but his dilatory habits were little suited to the wants and impatience of astronomers; this, and their own instrinsic merits, speedily advanced the brothers in their profession, and in the estimation of competent judges. Considerable rivalry ensued, and Edward Troughton, who felt his own powers, was not a person to conceal his feelings or to propitiate an adversary. After the death of his brother John, Edward alone continued the business till 1826*, when his increasing age and dislike to routine employment, induced him to take Mr. William Simms as his partner and successor.

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* The parish register for this time was destroyed.

"The life of an artist is generally contained in the history of his works, and this is peculiarly true of Mr. Troughton. Of him it may be said, with truth, that he improved and extended the use of every instrument he touched, and that every astronomical instrument was in its turn the subject of his attention. In this branch he has no equal, except the celebrated Graham, the object of his unbounded admiration. To describe all his improvements or inventions would require a detailed account of almost every instrument, astronomical, nautical, or geodæsical, in actual use; we must, therefore, limit ourselves to a brief and very imperfect notice.

"The instruments which facilitate navigation were peculiar objects of interest to Mr. Troughton; and long after his infirmities were an effectual bar to the applications of his most esteemed friends, he exerted himself to supply the seamen with well-adjusted and accurate sextants. '*Your* fancies,' he would say, 'may wait; *their* necessities cannot.' In 1788, he took out a patent for the double-framed sextant, a construction which, combining firmness and lightness, yet admitted of a considerable radius in this invaluable instrument. In the various adjustments of his sextants, the selection of their mirrors and glasses, he was most scrupulous, and for many years scarcely any other were seen in the hands of the most scientific navigators of our own or foreign navies. But there is a fault in the construction of the sextant, which the maker can only imperfectly guard against—that of a sensible excentricity; and the detection and correction of this, though not very difficult, is beyond the skill of ordinary observers. After trying and rejecting the repeating reflecting circle of Borda, Mr. Troughton (1796) hit upon one of his happiest constructions, the British reflecting circle, which bears his name,—an instrument which, in right hands, is capable of wonderful accuracy. The additional weight of the circle, and the trouble of the extra readings, have hitherto prevented its introduction into common use, and perhaps at *seā*, where the errors of observation are necessarily of as large an order as those of a good sextant, the superior accuracy of the circle may not come into play; but for observations on shore, and with a stand, no sensible observer can hesitate between the instruments. It is a characteristic trait of Mr. Troughton, that in order to bring his favourite circle into general use, he reduced its price below the usual profits of trade; and if he had succeeded in his attempt he might have been ruined by his success, for his sextants were by far the most gainful article of his business. With the same earnestness to promote navigation, he invented the dip-sector (afterwards reinvented by Dr. Wollaston,) and expended time, money, and ingenuity to no inconsiderable amount, in attempting to perfect the marine top for producing a true horizontal reflecting surface at sea. The marine barometer, the snuff-box sextant, and the portable universal dial, owe to him all their elegance, and much of their accuracy. Where others invented or sketched, he perfected.

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* The last capital instrument made by Mr. Troughton alone was a seven feet *transit for Sir J. South, in 1820*, on the model of that at Greenwich.

"Among ordinary physical apparatus may be mentioned, considerable improvement in the construction of the balance, the mountain-barometer, and the form given to the compensated mercurial pendulum, his pyrometer, by which some very valuable expansions have been determined; and the apparatus by which Sir George Shuckburgh ascertained the density of water, and that which, in the hands of Mr. Baily, has given the true length of the simple seconds pendulum. On the length of the simple pendulum Mr. Troughton himself made many very careful and extensive experiments, according to views and after a procedure of his own; but his ideas, though ingenious and elegant, were not pursued after Captain Kater's happy application of Huyghens' theorem. In the ordinary geodæsical instruments, Mr. Troughton greatly improved the surveying level and staff, and reduced them both in weight and price with increased convenience and accuracy. The errors of the common surveying chain, which are sometimes enormous*, and the complexity and expense of that made by Ramsden for the trigonometrical survey, led to the construction of a more simple and accurate chain, at a reasonable cost, and of easy use. The larger theodolites by Mr. Troughton, those, for instance, of twelve inches diameter, are remarkable for simplicity and power. In the refined and delicate instruments which have been applied to the most accurate geodæsical measurements, we may mention the large theodolite for the American coast-survey (1815), and those for the Irish (1822), and for the Indian surveys (1830), which may be advantageously contrasted, for their design and simplicity, with those, however otherwise excellent, of Ramsden; and also the apparatus for measuring a base line (1827), employed by Colonel Colby in Ireland, and Colonel Everest in India (1829). It must not, however, be forgotten, that the idea of this last most exquisite apparatus, and very much of the novelty of the construction, are due to our excellent members, Colonel Colby and Lieutenant Drummond. Mr. Troughton made some very beautiful and manageable zenith sectors, which were capable of great accuracy, considering their dimensions: one of these is, we believe, in the possession of the Danish gevernment, and was employed in the Holstein survey by Professors Schumacher and Gauss. He had designed one with a six-feet telescope and arc for America on the same construction, but it was never completed, though considerable progress had been made.

"It is, however, more properly with the astronomical instruments of this great artist that we are immediately concerned; and here he reigns without a rival. In the small altitude and azimuth circle (1792), the portable transit, and the portable universal equatorial, and the theodolite, it is not easy to state accurately the line between his improvements or inventions, and those of preceding artists; but it is from him that these instruments received their present form and perfection. The repeating circle of Borda, an instrument which he disliked, first received its beauty and accuracy from his hands; and for it he invented his very elegant double-foot screw, to give minute adjustments. The ordinary reading micrometer, and the position micrometer commonly employed in the measurement of double stars, were greatly improved by him in simplicity, and brought to perfection; and he first applied the former to dividing. In the class of larger altitude circles, revolving freely in azimuth, we find the circle for Count Bruhl (1792), the Westbury circle (1806), that for Sir T. M. Brisbane, the Society's Lee circle, the substance and form of Dr. Pearson's circle, those employed in the Indian survey for determining latitudes (1830), and that at the Edinburgh observatory (1830), with many others which it would be tedious to mention. In Groombridge's transit circle (1806), the beauty of the form and accuracy of the divisions satisfied every body but the artist himself*, whose experience and matured powers were finally exhibited in the mural circle. This finished specimen of sound engineering and mechanical construction at first met with much opposition; and many were the objections, both before and after the erection of the Greenwich mural (1812), with which its inventor was assailed. To these he resolutely turned a deaf ear, for nothing could shake his confidence in what he *knew;* and he had the pleasure of finding, before his death, the mural circle established in the Royal Observatory of Paris, and safely lodged on its massive pier at the Cape, St. Helena, Madras, Cracow (1832), Edinburgh (1834), Brussels (1835), Cadiz, Armagh, and Cambridge (1832). A small model, of two feet in diameter, was made for Sir T. M. Brisbane prior to the Greenwich mural, and is now at the observatory of Paramatta. The principal equatorial instruments of Mr. Troughton, with accurately divided circles, are those of Coimbra (1788), Armagh, and Brussels (1834). When we add to this long catalogue the large transits at Greenwich (1816), Camden Hill (1820), Cracow (1828), Markrea (1832), &c., in which the differential screws are so beautifully applied to form what he called the *bones* of the instrument, and the gigantic zenith tube at Greenwich, which he just finished before his strength failed, we have reason to wonder that even his long and active life should have sufficed for works of such variety and extent, and may form some estimate of the value of Mr. Troughton's labours, and their effect on modern astronomy. It may, perhaps, be remarked, that the only astronomical instrument which is not greatly indebted to Mr. Troughton is the *telescope;* and he was deterred from any attempt in this branch of his art, by a singular physical defect, which existed in many members of his family. He could not distinguish colours, and had little idea of them, except generally as they conveyed the idea of greater or less light. The ripe cherry and its leaf were to him of one *hue*, only to be distinguished by their form; and he was in the habit of relating some curious mistakes committed by himself, and others of his relations, in confounding green and red. With this defect in his vision, he never attempted any experiments in which colour was concerned; and it is difficult to see how he could have done so with success.

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* On one occasion when two surveyors differed in their measurements their *chains were sent to* Mr. Troughton to compare; one was found two inches too short, *the other three inches* too long.

"The most remarkable of Mr. Troughton's writings are, 'An account of a method of dividing astronomical and other instruments by ocular inspection,' &c., printed in the *Phil. Trans.*, 1809, which was rewarded with the Copley medal; 'A comparison of the repeating circle of Borda with the altitude and azimuth circle,' in the 1st volume of the Society's *Transactions;* and several articles in Brewster's *Edinburgh Encyclopædia*, such as 'Circle,' 'Graduation,' &c. The descriptions of instruments of his invention or construction, may generally be considered to have been furnished by him to the authors of the articles in which they appear; and an experienced judge will easily distinguish by the luminous conciseness of his style (for he wrote in the same spirit as he constructed), the cases, and also the extent, to which this remark applies. His method of dividing has been generally adopted, and with the obvious modification of dividing by diameters instead of radii, so as to eliminate any defect in the turning of the collar or rim, has not received, and seems scarcely to require, any further improvement. In 1825, Mr. Troughton paid a visit to Paris, where he was received with great cordiality and respect by the distinguished artists and men of science of that metropolis. In 1830, he received an honorary gold medal from his majesty the King of Denmark.

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* Mr. Troughton was, upon reflection, so little satisfied with the design of this transit circle, that he broke up a second, on a somewhat smaller scale, after he had spent 150*l.* upon it. "I was afraid," he said, "I might grow covetous as I grew old, and so be tempted to finish it, and I don't think it is a good kind of instrument."

"Mr. Troughton was one of the original members of this Society, and at all times sincerely anxious for its prosperity. So long as his health permitted, he continued to be a constant attendant, notwithstanding his deafness, on the meetings of the council, and the ordinary meetings of the Society; and his valuable assistance and advice were most readily afforded whenever they were required. His singularly clear understanding, his unimpeachable integrity, and kind, though independent temper, made him universally respected; and there was an originality and raciness in his conversation and anecdotes rarely to be met with. At times, his criticisms might be considered to be severe, but never unfair, except, perhaps, on his ancient antagonist Ramsden, against whom he fancied he had grounds for complaint. In every other instance he was as ready to praise as to censure, and gave full and hearty approbation to the masterly conceptions of Graham, the beautiful execution of Bird, and the ingenuity and fertile invention of Ramsden himself. Nor was he niggardly towards his contemporaries and juniors. It may be added, that notwithstanding his high reputation and very simple habits, his carelessness of money did not allow him to become rich, for his object was fame rather than wealth.

"Mr. Troughton died at his house in Fleet Street, June 12, 1835, in the eighty-second year of his age, and was buried, according to his request, at the cemetery Kensall Green. Many of the members of this Society paid their last tribute of respect to his memory by attending his funeral.

"An admirable marble bust of Troughton, by Chantrey, was executed some years before his death, at the expense of his private friends and admirers. This is placed, as he wished it to be, in the Royal Observatory, Greenwich."

Those who did not know him well, or have only seen Mr. Troughton in his dark, dusty, back parlour, in Fleet Street, surrounded by books and instruments, which had never been moved perhaps for years, himself scarcely less antiquated in his dress and appearance than the furniture around him; will perhaps smile with surprise or contempt, when we say, that in our mind, he was always the personification of the chivalry of the middle ages, without its vices: to a courage which would honour a soldier in the breach, he united the courtesy, the generosity, the humanity, and the romance of that period;—the term has become degraded by its prostitution, or else we should say he was the perfect *gentleman* in all the olden signification of that once expressive word. Conscious of his deafness, he could sit for hours in crowded society, waiting till those around addressed him, never manifesting any irritability at apparent neglect; he was holding communion with his own mind, and rich must have been that which could have afforded pleasanter intercourse. When importuned by the querulous complaints of his disappointed customers, or by the superficial remarks of would-be mechanicians, his bland features betrayed no impatience nor contempt, he "shifted his trumpet and only took snuff." Towards the female sex he manifested that refined delicacy which ensures their favour and esteem; and children loved him, for he could amuse and converse with them as a parent. When oppressed by sickness and the infirmities of age, and feeling the approach of death, which he regarded with equal resignation and composure, his countenance would lighten up with pleasure at the entrance of a friend, and beam with intelligence at any happy remark or entertaining anecdote: in short, if ever man possessed perfect command of temper, united to acute susceptibility, it was Edward Troughton.

The biographer in the report has mentioned that he was not rich—no wonder, for every season he assembled around him his less-prosperous relatives, and shared his annual profits among them,—he was too proud to accumulate wealth, and even those fair advantages which the most conscientious in trade would unhesitatingly avail themselves, he scorned if they were at variance with his refined opinions of what was consistent with honour. His failing, as has been remarked, was love of fame, and much do we lament that no greater honours have been paid him, than such as courtesy or friendship suggested at his funeral.

CAPTAIN HENRY KATER,

"formerly of his majesty's 12th regiment of foot, and latterly on the half-pay of the 62d, was early imbued with a taste for mechanical and philosophical inquiry. Having suffered considerable constitutional injury from the climate of India, where he had been stationed, he returned to England, and devoted his time and attention entirely to scientific objects.

"That Captain Kater was both a delicate experimenter and accurate observer, is abundantly proved by the various papers which are published in the *Philosophical Transactions*. They consist of suggestions for the improvement of astronomical instruments; experiments relative to the pendulum; inquiries respecting a standard of weights and measures; trigonometrical operations for determining the difference of longitude between the royal observatories of Paris and Greenwich; investigations of the magnetic forces; and details of his invention and application of the horizontal and vertical floating collimators.

"The pendulum-experiments had occupied Captain Kater's close attention for many years, and he has permanently attached his name to the well-known property of the reciprocity of the centres of oscillation and suspension, and their consequent quality of convertibility. Though this was a property already known to belong to the centre of oscillation, it had never hitherto been practically applied to determine the exact length of a pendulum vibrating seconds; it was, therefore, highly creditable to his ingenuity, and claims the merit of an original invention. And in this, as well as in his laborious inquiries respecting a standard of weights and measures, even where his conclusions have not escaped all the chances of error, he has led the way to the still more delicate researches which have followed.

"In the expedition which sailed, in 1818, for the discovery of a north-west passage, it appeared that from their near approach to the magnetic pole the compasses on board had become nearly useless. This induced Captain Kater to make a series of experiments to ascertain the best kind of steel, the form of needle most calculated to receive the greatest directive energy with the least weight, and the most effective mode of magnetizing it. The results led to the adoption of the *shear* clock-spring steel, and the *pierced rhombus* form, in the proportion of five inches in length to two inches in width. The azimuth compasses thus fitted have afforded very accurate deductions, and have therefore been much used in surveying. This instrument is the more valuable, from being fitted with a *sight*, on which slides (in a frame) the segment of a glass cylinder, ground to a radius of five inches, by means of which a fine line of light is thrown on the index, and may be seen at the same time as the graduations on the card. This ingenious and simple application appears to have been known to Halley, and is, indeed, described by the celebrated Godfrey, of Philadelphia; yet it had been suffered to be forgotten for more than a hundred years.

" By the application of optical and hydrostatic principles to the construction of his collimator, Captain Kater intended to determine the situation of the line of collimation of a telescope attached to an astronomical circle, with respect to the zenith or the horizon, in any one position of the instrument, and thereby to obviate the necessity of using either plumb-line or level. The vertical collimator was an improvement on the horizontal one, in obviating the sources of error arising from transferring the instrument to different sides of the observatory, and of taking the float out of the mercury and replacing it at each observation. As this appeared an object beneficial to practical astronomy, its inventor was awarded the gold medal of this Society in 1831.

" Captain Kater's health had long been declining, and latterly a complaint in his eyes debarred him from his usual pursuits, his last experiment having been made on this Society's Standard Scale. Sinking under a severe affection of the lungs, he died at his residence, York Gate, on the 26th of April, 1835, in the fifty-third year of his age."

Not having had the pleasure of more than a public acquaintance with the first and the last of these savans, we have no observations to make on the sketches of their lives furnished by the report; but we cannot help smiling at a trait of inconsistency in a Learned Society, which we recollect with regard to the Bishop of Cloyne. It presented to Mr. Pond, the late Astronomer Royal, the Copley medal for observations which rendered doubtful the accuracy of Dr. Brinkley's alleged discovery of a parallax in the fixed stars, and in the year following, it voted to the learned doctor the same honorary distinction for that very deduction!

MISCELLANEOUS INTELLIGENCE.

Description of a Self-adapting Pipe for drawing off Liquids, at a constant Depth from a Surface of variable Level.

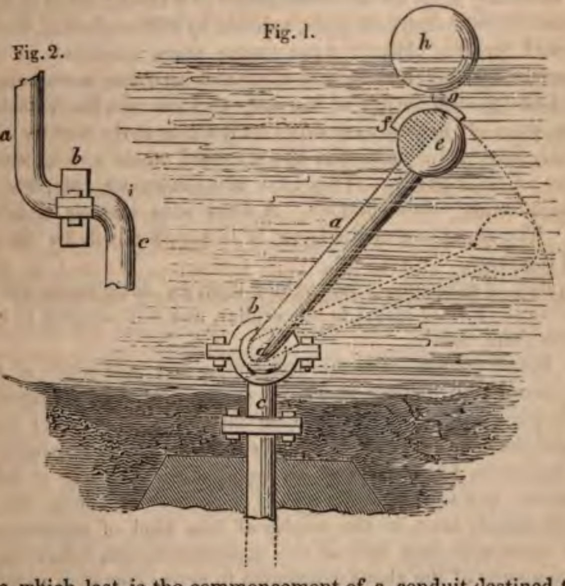
Agreeably to your request, I send a drawing and description of my recent improvements and suggested applications of an apparatus which I introduced, about twenty years ago, into the Howard system of sugar-refining (a slight sketch of which was published in 1827, in the *Repertory*). The object being to draw off liquor at a level always below the floating scum, and above the subsiding feculence, so that it might pass to the filters with the least possible quantity of impurity and thus restore the stoppages necessary to clean them. And I the more readily accede to your request, since I have long thought the idea might be peculiarly applicable in those cases where water is drawn from turbid, and particularly tidal rivers, for the supply of towns; it is obvious that the purest water will be obtained from the greatest altitude above the bed of the river, provided it be taken so much below the surface, that floating substances shall not be drawn into the pipe.

As it is desirable that the attention of the Metropolitan and other Water Companies, should be called to every plan offering the chance of their obtaining the clearest water which their respective localities may afford, it is hoped that they will give this improvement due consideration, at least.

Fig. 1 is an elevation of the Self-adapting Pipe, in action in a River. A pipe *a*, of any required diameter, is attached at its lower end by a joint *b*, to a fixed pipe *c*, which last is the commencement of a conduit destined to convey the water away. By means of this joint, the first pipe *a*, can move in a vertical plane, like an arm or radius, round the centre *d*. Its upper end terminates in a spherical *nozzle* *e*, which is connected by a rib *f*, and a chain *g*, which may

Fig. 2.

Fig. 1.



be lengthened or shortened at pleasure, to a light hollow ball *h*, floating on the surface, A B, of the water. The nozzle is perforated with small holes, on the part which becomes uppermost when it is at its greatest depression. The other part is preserved solid, so that, if the nozzle rest on the bottom, the mud may not enter the pipe.

Fig. 2 is a side elevation of the joint. The elbows *i* and *j* on the two pipes *a* and *c* are necessary in order that the adapting-pipe may move in a vertical plane.

It will be evident on inspection that by means of the chain *g*, the nozzle *e*, may be set to any depth desired, below the surface A B, and that when this is once done, it will constantly remain at this depth, however variable the level of the surface may be.

In cases where the mud at the bottom might be so deep that the nozzle might sink to the perforations, a stop might be easily attached, to arrest it at any desirable level.

A comparison of this apparatus with the wretchedly-contrived dolphins by which the water-companies pretend, at present, to take clean water only from the Thames, would at once show its superiority, and the necessity of something as efficient being substituted for these old blunders.—*Extract from a Letter of John Isaac Hawkins, Esq., Civil Engineer, to the Editor. February 26th,* 1836.

Geological Map of France.

THE Board of Mines, in France, have been, for many years, commissioned to take the necessary steps for the construction of a Geological Map of that kingdom. Some distinguished geologists, assisted by able engineers, were appointed by the board for this object. After twelve years employed in visiting and examining the country, and in collecting and verifying materials, they have completed the general outline of the map, and inserted the boundary lines of the strata, precisely as they were found on the face of the country. In addition, they have indicated the position of the principal mines and quarries, and the boundaries of their grants. They have also drawn up complete accounts of all the metallurgic establishments. A descriptive memoir is to accompany the map. The engraving of the latter it is supposed will be finished in the course of the year.

A system of colouring is determined upon for designating the strata and their varieties. This is stated to be peculiar, and well adapted to indicate and distinguish them clearly,

The Board propose, after the completion and publication of this general map, to proceed with distinct geological maps of all the Departments. Into these, a larger quantity of detail will be introduced. In order that no time may be lost, the board are now furnishing funds to enable provincial geologists, mining engineers, &c. to carry on surveys and explorations in different parts of the country, and to publish the results of their observations. So active have the Board already been in the employ of this kind of patronage, that it is expected more than one half of the departments will, before the end of the year, have agents of the above description in active operation within them, the whole having for their object the systematic formation and accomplishment of a grand series of maps displaying minutely the topographical geology of France.

New mode of reducing Silver, &c.

M. Becquerel presented to the Academy of Sciences several electro-chemical apparatus, by which he had effected the immediate reduction of silver, lead, and copper.

"I am able," says he, "without the intervention of mercury, by constructing an electro-chemical apparatus of iron, a concentrated solution of common salt, and an argentiferous mineral properly prepared, to extract from the last the silver which it contains, in the form of crystals. The minerals experimented upon are those which are worked in Colombia, and Allemont. The same process will reduce from the copper pyrites of Chessy, near Lyons, the silver which they contain, without touching the copper. Till the present time none but the argentiferous galenas have been successfully treated, though with considerable difficulty, for the extraction of the silver.

"When a mineral, like that of Allemont, contains several metals, as lead, copper, &c., each metal may be reduced separately and at different periods, in such a manner that the separation goes on naturally. It follows from this that the minerals of copper and of lead can be treated in the same way as those of silver, but with much less facility, on account of the different degrees of oxidation which they take, and of the compounds which form during the roasting.

"As the researches on the extraction of metals with which I am now occupied, will occupy a long time," observes M. Becquerel, in conclusion, "I have thought it proper, for the interest of science, to make known to the Academy the principle by the aid of which some of the metals may be extracted from their respective minerals, and particularly silver."

Medal in honour of Watt.

". . . . . I called on M. Galle a few days since. It was my first interview with the medallist, I was, however, soon led by him into his *Atelier*. An indifferent cast of Watt, by Chantrey, and an engraving, by Wagstaff, from Sir William Beechey's picture of the same eminent man, occupied positions which marked they had been objects of the artist's recent attention, if not of his study. We were almost immediately engaged upon them. M. Galle was disposed to be severe upon some anatomical defects which he pointed out in the forehead of the bust, and referred to the picture to corroborate his criticism. My remark, that the cast might not be a perfect one, (and I really thought it so,) was not admitted as a justification. Soon after, M. Galle produced a study in wax, of the head of Mr. Watt, which he had recently begun, and observed, 'More than twenty years ago I designed and executed a medal of Mr. Boulton, of Soho, the partner of Mr. Watt; upon this medal I bestowed great care, here it is:' producing a large highly-finished bronze medal. 'I had forgotten it for many years. A few weeks since I was so excited by an eulogy on Mr. Watt, which M. Arago pronounced at the Institute, that I felt disposed to undertake a medal to his honour. I then recollected my Boulton; I drew it again to-day, and I began a *pendant* in Mr. Watt. You see how far I have advanced.' On examination, I found that a very successful and considerable progress had indeed been made by the veteran artist. I now felt again, and deeply, the defects in the cast, and returned to them. M. Galle only remarked, that as the medal was not an order, but, like his Boulton, undertaken at his sole cost, he *could not afford to send to* England for another. Perhaps, I thought, if this were known, some Englishman, who had the means, and who could value this voluntary tribute of respect to one of his countrymen from a distinguished foreigner, might furnish M. Galle with better materials for his noble purpose." *Extract from a Letter of a Friend of —— to the Editor. Jan.* 1836.

Unaccountable Theft of Chemicals by Rats.

About two years ago, in the warehouse of Mr. Johnson, Chemical-manufacturer, in Hatton-Garden, 50 oz. of Oxide of Uranium were put into as many half-ounce bottles, each bottle wrapped in paper, and put into a drawer, in a counter. The premises having been injured by an accidental fire, the floor of the room in which the oxide was kept was taken up, about six weeks ago: between the floor-boards and the ceiling of the room beneath, were found deposited, twenty-eight of the above bottles, and two others. The paper wrappers had been removed, and the outsides of the bottles were dirty, but the corks were sound, except a few which had been slightly nibbled, and the contents of the bottles were untouched. The other two bottles, containing Tungstic Acid, were also found corked, and untouched. The removal of these bottles had been effected by rats. The counter was nearly destroyed by the fire, but the workman who made it recollected that it had no back-casing, and that the oxide-drawer did not go close up to the division which separated it from the drawer above; so that a long aperture between them was left; through this the rats had entered. They then must have lifted the bottles, passed them through the aperture over the back of the drawer, and dropped or lowered them down to the floor, and afterwards dragged them to their deposit.

But what was the inducement to commit the robbery? The oxide of uranium is inodorous and tasteless, though of the latter quality they could not be aware, as all the bottles were found tightly corked, and the enclosed quantities were evidently the same as when put into the drawer.

A deficit in the oxide had been observed, but the amount had never been exactly ascertained before the fire happened which drove the thieves from their retreat, and was the means by which the owner recovered the stolen property.

Appearance of the Cross-Bill in Fifeshire.

The Cross-Bill (*Loxia curvirostra*, Linn.) has been seen in Fifeshire, on the north side of the Frith of Forth. Its appearance in this country is extremely rare, as, it is believed, it has not been seen since the severe Winter of 1825. Its name is derived from the remarkable conformation of its Bill, the mandibles crossing each other at the points. Its principal food consists of the cones of pine trees, and in breaking them the utility of the peculiar formation of its bill is seen. The individuals that were observed were by no means shy, remaining undisturbed till they were approached quite close. They were supposed to have come from Norway, where its food is very plentiful.——R. M., *Edinburgh, March* 11th.

Easy Fusion of Platinum in Large Quantities.

"If platinum be fused upon charcoal it will be brittle, and unfit for the purposes to which it is usually applied. I have no doubt it is converted into a *carburet*. I have tried several substances as a support for it, of which I find Stourbridge clay the best. Mr. Johnson, of Hatton Garden, has been kind enough to witness some experiments with the blowpipe, and he suggested the use of the above material as a support. He brought with him some bone-ash cupels, but they were readily fused. All substances, indeed, seem to fuse under the intense heat produced by the combustion of the gases* in question. I find the best mode of fusing platinum is to keep adding gradually to the fused mass small pieces of the metal. When an ounce or more has thus been acted upon, the metal will be in fusion at the surface, but will become solid at the bottom. We may thus go on welding or agglutinating the platinum to any extent. Before rolling, or using the metal in any other way, be careful to cut off that end which was next the support, as this becomes incorporated with a portion of silica, which renders it unfit for working. By adhering to the principle laid down, by having gasometers sufficiently large (and their size may be increased to any extent with perfect safety), and by having the orifice whence the gases issue augmented to a considerable extent, I am convinced platinum may be fused in almost any quantity. I have succeeded in agglutinating more than half a pound of this metal by the process just described." —Mr. MAUGHAM's *Communication to Society of Arts, May* 12th, 1835.

Standard Scale and Standard Yard.

AMONG the subjects on which your Council has to congratulate the Society, none more peculiarly relates to ourselves than the successful determination of a Standard Scale, which has been, in the last two meetings, so ably reported by Mr. Baily. At first sight, scarcely any operation could be imagined more easy or more simple than that of making one straight line equal to another straight line; and it is only after a careful perusal of Mr. Baily's report, and a consideration of the accuracy which is demanded in such determinations when employed in deducing the figure of the earth by the actual measurement of degrees, or in ascertaining the absolute length of the seconds' pendulum, that the difficulty of the task or the importance of the result can be appreciated. After many struggles with minute and unforeseen sources of error, which had escaped the notice of former observers, Lieutenant Murphy, R.E., and Mr. Baily †, have put the Society in possession of a *Standard Yard* and a *Standard Scale*, as accurate as human senses, armed by the best means which science has hitherto contrived, can produce. In pursuing this object, Mr. Baily and Lieutenant Murphy received ready assistance from many members of the Society; and the apparatus, made by Messrs. Troughton and Simms, is admirably contrived for convenience and accuracy.

The interest and value which would in any case have attached to this subject, has been much enhanced by the destruction of the Imperial Standard in the late disastrous fire which consumed the two Houses of Parliament. For the history of the origin of these standards, and a description of them, Mr. Baily's report must be consulted; we may however remark, that a single comparison with the neat divisions and elegant mounting of the Scale of the Astronomical Society, would give a more accurate determination of length, than a day's work would have done with the clumsy and awkward original; while the immense number of comparisons between the two scales, directly and indirectly, under every variation of observers and circumstances, throughout several weeks, has ascertained their relative lengths with perfect accuracy.

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* Oxygen and Hydrogen.

† Lieutenant Murphy undertook to make the comparisons between the Imperial Standard and the Society's Scale, but before the results could be obtained, he joined the expedition to the Euphrates under Colonel Chesney. Mr. Baily executed the remainder of the work, and drew up the report.

It is undoubtedly the most exact copy of an original that was ever taken; and as fac-similes of our Standard were made at the same time for the Danish and Russian governments, and copies have since been taken for Mr. Baily and Mr. Simms, there is no reasonable fear that the measure should be lost or vitiated. Your Council trusts, that for the future every British measure of length, at least for scientific purposes, may be expressed in terms of this unit. Mr. Baily's memoir contains a very curious and elaborate history of the various standards which have been in use in this country, and a comparison between our yard and the *mètre à bouts* and *mètre à traits* in the possession of the Royal Society.—*Report of the Royal Astronomical Society.*

Premiums offered by the Royal Cornwall Polytechnic Society, for 1836, *and not confined to the County.*

A PREMIUM of Ten Guineas, by Sir Charles Lemon, Bart., and R. W. Fox, Esq., for the best series of practical experiments, tending to prove how the dangers attendant on the present mode of blasting rocks, may be most effectually and economically guarded against.

Two premiums, the first of Seven Guineas, the second of Three Guineas, by G. S. Borlase, Esq., for the best and second best chemical or mechanical plans for ventilating mines, which can be applied to the Cornish mines with advantage.

A premium of Ten Guineas, by G. C. Fox, Esq., for the best Essay on the various diseases incidental to miners, their causes, and the best practical means of remedying them. Any statistical information as to the longevity of miners, compared with that of the other population of the county, will be deemed highly desirable.

A premium of Ten Pounds, by John Hearle Tremayne, Esq., for the best available method, or improvement on the plans already suggested, for facilitating the ascent and descent of miners, provided the judges shall consider it to possess sufficient merit to be entitled to the premium.

A premium of Ten Guineas, by E. W. W. Pendarves, Esq., for the best practical method of ascertaining the quantity of water raised by each lift of pumps in the mines of this county.

Two premiums, the first of Seven Guineas, the second of Three Guineas, by John Taylor, Esq., for the most complete and accurate accounts of the quantity of water supplied to the boilers, the number of bushels of coals consumed, and the duty performed by any engine, for a period of not less than six months in the ensuing year.

A premium of Ten Pounds by the Rev. Canon Rogers, for the most economical, safe, and efficient plan for lighting mines, consistent with the health of the miner:—such plan to be accompanied by a statement of the present actual consumption of candles, and the cost per dozen lbs., at some of the principal Cornish mines.

Two premiums, the first of Ten Pounds, the second of Five Pounds, by H. H. Price, Esq., of London, Civil Engineer, one of the Honorary Members of this society, for the best and second best practical plans for adapting to steam-vessels the method used in Cornwall, of working steam expansively; including practical drawings of the construction of the boilers and expansion-gear. Such boilers should combine economy of fuel with safety, both as regards the danger from explosion, and accidents to the vessel by fire; with suggestions as to the best method of preventing the loss of heat by radiation, or otherwise.

Due regard must be had to the essential difference between a single-acting engine working pumps by a lever, and two double-acting engines working a crank.

All plans should be accompanied with accurate models or drawings, estimates of expense, and all the information necessary to enable the judges appointed for the purpose, to form a correct judgment of their respective merits.—Should several plans for any premium be proposed by the same person, a model of one, with accurate drawings and estimates of the others, will be deemed sufficient: no individual to be entitled to more than one premium, where two are offered for the same object.—The judges will be requested to withhold any of these premiums, provided no plans be brought forward, which they shall deem of sufficient importance to merit them; the premiums will then be continued to another year.—Competitors for the foregoing premiums are requested to send their plans, &c., *free of expense*, to the secretaries, on or before the 1st of August, 1836 ——*Secretaries*, Mr. Lovell Squire, jun., and Mr. Thomas B. Jordan, *Falmouth.*

Railroads and Locomotive Trains in Bavaria.

On the 7th of December last, a railroad, which had been completed from Nürnberg to Furth, in Bavaria, was opened to the public with great ceremony. The magistrates and other authorities of the towns, and a large number of spectators having surrounded a monumental stone erected in honour of the completion of the undertaking, the burgomaster of Nürnberg began the proceedings of the day with an address. The band of the civic militia then played the National Hymn, the stone was uncovered, and one side displayed the cipher of the present king of Bavaria, and beneath it the inscription—"Deutschlands erste Eisen-bahn mit Dampkraft, M.D.CCC.XXXV*." On the opposite side were seen the united arms of the two towns, and the inscription, "Nurnberg und Furth." After a short pause, the Locomotive Steam-Engine, with nine carriages, decorated with the national colours and filled with passengers, started, and reached Furth safely, in about fifteen minutes. Several other trips, backward and forward, were made during the day.

Instance of Human Effort.—Six Days' Sawing.

A pair of sawyers, in the yard of Messrs. Paul, and Co., timber-merchants, Broad-street, Golden-square, executed the following quantity of labour in sixty working-hours, in six days, beginning about eight, A. M., on Monday, the 25th, and ending about four, P. M., on the following Saturday, the 30th of January, in the present year.

They sawed through an area of 3068 square feet of American Pine, along a line whose total length was 1726 feet. In doing this, they raised the saw 124,272 times, and as this tool weighed 30 lbs., they lifted an actual weight of 3,728,160 lbs. But this amount of labour was not more than one-third the actual exertion expended; for to overcome the friction, in pulling up the saw through the kerf, and forcing it down again through the wood, at least two thirds more was necessary; the total labour, therefore, was equal to lifting 11,184,480 lbs. to the height of the stroke, and as this was four feet, there was 44,737,920 lbs.=19,958 tons. 18 cwt. raised one foot high, in 60 hours, which is 12,427 lbs. = 5 tons, 11 cwt. raised one foot high, per minute, by the two men, and 2 tons, 15½ cwt, per man, 1 foot high per minute.

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* "Germany's first Iron Rail-road with Steam-power."

To estimate accurately this remarkable week's work, it should be known that there was no preparation whatever on the part either of the sawyers or their employers. On the Saturday preceding, the men, being upbraided for having done little, boasted they would earn 6*l*. in the week ensuing. One of their masters merely observed such an amount had never been earned yet, and promised them a gallon of ale if they did it. The lot of work had been previously marked out,—it was by no means favourable to the men, either in kind or size, and was to be cut both into planks and boards,—they lined the pieces of timber and got them on and off the pit,—they shifted the transom,—and sharpened the saw,—in short they did everything just as usual, and had no assistance whatever; they worked no extra hours, and took no unusual stimulus, drinking three pints of porter each, per day.

The work was witnessed, as it proceeded, by workmen and others in the yard, and was certified, by these eye-witnesses, as having been done. It was also measured by a third party, between the men and Messrs. Paul and Co., and the latter paid 6*l*. 2*s*. 7*d*. to the men for the week's work. Four pounds is considered, in general, a good week's earnings, for a pair of sawyers, in London, on similar work. More than 4*l*. 10*s*. per week is seldom got by the best hands, on the best work.

The top-sawyer, William Thompson, is a native of Birmingham; he was weighed after the work, and drew 11 stone, 13 lbs.—his height is 5 ft. 8¼ in. He completed his thirty-fifth year in a fortnight after. The pit-man, Daniel Hughes, was born at Winchester; he weighed, at the same time, 12 stone, 3 lbs. and measured 5 ft. 9¼ in. He was forty-six years old on the Friday during the job: both are married and have families. Thompson was a little distressed on the Saturday. Hughes was as fresh as when he began.

This feat having been noticed in the newspapers, and turning out, upon inquiry, to be true, it was thought worth the trouble to ascertain all the particulars, from authentic sources; we applied, therefore, to Messrs. Paul and Co., who introduced us to the men, and enabled us to make the preceding statement.—ED.

Double Sextant.

A Double Sextant, by Mr. Rowland, of London, has lately received the approbation of the French Academy of Sciences, after a report had been read upon it, from a committee composed of MM. Arago, Mathieu, Beautemps-Beaupré, Puissant, and de Freycinet. This instrument has been patented in England, and subsequently in France.

Preservation of Corn in Granaries.

THE destruction of corn in granaries often exceeds the enormous amount of 14 per cent. in a year. This is principally owing to two causes:—1st, the presence of weevils, &c.; 2nd, the heating of the corn itself.

Taking these for data, and guided by a series of experiments which he had instituted for the purpose, M. Valley has designed and presented to the *Académie des Sciences*, an apparatus which he believes will prevent this two-fold cause of loss.

His experiments have led him to the conclusion, that weevils are fond of *quiet, and not* disposed to the propagation of their species, unless under circumstances of perfect tranquillity, and a certain condition of atmospheric temperature; that, therefore, if they are disturbed in the smallest degree, and the air has not the proper temperature, or even if the latter be made to circulate among the grain in the great heats of Summer, so far from increasing, they will instantly decamp.

M. V.'s experiments have also proved, that if a heap of corn be moistened so as to be in the most favourable state for fermenting, this disposition will certainly be removed if the mass be agitated, and at the same time be exposed to the action of a current of air passing through it.

Patent-Law Grievance.

In the fraction which has passed of the present year, the inventive genius of the nation has already been amerced in nearly £10,000*l.* Contrast this with the rational and just conduct of the Prussian Government. This grants an equal, if not superior amount of protection, at the cost of about 50*l.*; and besides, advertises the invention of the patentee, without any additional cost to him, once in every newspaper in the kingdom! This exorbitant amount of fees for letters-patent is, we hope, about to be reduced. Notice was given in the House of Commons, on Wednesday, the 23d ult, by Mr. Mackinnon, that he should move on the 21st instant, for a "Select Committee, to consider the expediency of amending the present system of granting patents, and of rendering more easy and expeditious, and also less expensive and less liable to litigation, the manner of securing to individuals the benefit of their inventions."

NEW PATENTS.

GRANTS.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

FEBRUARY *cont.*

39. Clinton Gray Gilroy, Argyle-street, New-road, St. Pancras, *Middx.*, Engineer; for certain improvements in machinery for weaving plain and figured fabrics. Feb. 25.—Aug. 25.

40. William Gilyard Scarth, and Robert Scarth, both of Leeds, *York.*, Dyers; for manufacturing or preparing of a certain substance for blue dyers from materials not hitherto used for that purpose, applicable for dyeing blue and other colours. Feb. 26.—Aug. 26.

41. James Barron, Brass-founder, and Edward Thomas, Workman, both of Birmingham, *Warw.*, for improvements on bedsteads, and apparatus to be used with or for bedsteads. Feb. 26.—Aug. 26.

42. Robert William Sievier, Henrietta-street, Cavendish-square, *Middx.*, gent.; for an improvement in the means of dissolving and preparing caoutchouc or India rubber, for various purposes. Feb. 27.—Aug. 27.

43. James Martin, Charing-Cross, Westminster, *Middx.*, Gent.; for an improvement in dissolving and preparing caoutchouc or India rubber, to render it applicable to various useful purposes. *For. Comm.* Feb. 27.—Aug. 27.

Total, February...24.

MARCH.

44. William Bates, Leicester, Fuller and Dresser; for improvements in the process of finishing hosiery, and other goods manufactured from lambswool, angola, and worsted yarn. Mar. 8.—Sep. 8.

45. Charles Schafhautl, Sheffield, *York*, Gent.; for improved gear for obtaining a continuous rotary action. Mar. 8.—Sep. 8.

46. Anthony Theophilus Merry, Birmingham, *Warw.*, Metal-dealer; for the application of certain white metal plated to certain manufactures, to which it has not hitherto been applied. Mar. 8.—Sep. 8.

47. James Morison, Paisley, Manufacturer; for improvements on the *Jacquard* machine, and on what is called *the Ten Box Lay*, and in the reading and stamping machines used in making shawls and figured work. Mar. 8.—Sep. 8.

48. John Galley Heartley, Devonshire-street, Bishopsgate-st.-without, *Lond.*, Manufacturer of caoutchouc; for improvements in preparing or manufacturing caoutchouc or India rubber, for various useful purposes. Mar. 8.—Sep. 8.

49. John Godwin, Cumberland-street, Hackney-road, *Middx.*, Piano-forte maker; for an improvement in the making or construction of piano-fortes. Mar. 8.—Sep. 8.

50. Benjamin Simmons, Winchester-street, Southwark, *Surr.*, Engineer; for certain improvements in retorts, stills, and other chemical apparatus, and the machinery connected therewith, and by the use or employment of which, various processes can be more speedily, conveniently, and economically performed. Mar. 8.—Sep. 8.

51. George Holworthy Palmer, Canal-grove, Old Kent-road, Civil-engineer; for an improvement in the purification of inflammable gases, and an apparatus by which the improvement is applied; such apparatus being also applicable to other useful purposes. Mar. 8.—Sep. 8.

52. Charles Guynemer, Manchester-street. Manchester-square, *Middx.*, Professor of singing; for certain improvements in piano-fortes. *For. Comm.* Mar. 8.—Sep. 8.

53. George Lawrence, No. 9, New Bond-street, Hanover-square, *Middx.*, Dressing-case maker; for a certain improvement in the screws used for fastening the mouths of mounted-inkstands, perfume-, liquor-, and medicine-bottles, also in fastening the mouths of jars and tumblers used for paste, salve, powders, preserves, and other purposes. Mar. 8.—May 8.

54. James Diggle, Bury, *Lanc.*, Engineer; for certain improvements in steam-engines. Mar. 8.—Sep. 8.

55. Charles Watt, Clapham, *Surr.*, Gent.; for certain improvements in preparing, purifying, and refining tallow stuff, fatty materials, and animal and vegetable oils, for various useful purposes. Mar. 8.—Sep. 8.

Masters, Leicester, *Leic.*; for
uproved essence of anchovies.
14.—Sep. 14.

Chalklen and Thomas Bonham,
d-street, *Middx.*, Water-closet
facturers; for an improvement
provements on the instrument or
atus commonly known by the
of vices. Mar. 14.—Sep. 14.

ard Jelowicki, No. 8, Seymour-
Bryanstone-square, *Middx.*,
for certain improvements in
-engines. Mar. 14.—Sep. 14.

as Alcock, Claimes, *Worc.*, lace
facturer; for certain improve-
in machinery for making bobbin-
ce, for the purpose of producing
n kinds of ornamental bobbin-
ce, and other fabrics, by aid of
nprovements which are in part
able to machinery constructed
ling to former letters Patent
ed to him. Mar. 17—Sep. 17.

onsus William Webster, Re-
treet, *Middx.*, Aurist; for an
ment or apparatus to be applied
ear to assist in hearing. Mar.
Sep. 17.

Birkby, Upper Rawfolds, Leeds,
, Card maker; for improvements
achinery for making needles.
17.—Sep. 17.

Elizee Seignette, Mincing-lane,
, Merchant; for improvements
eserving animal and vegetable
nces. *For. Comm.* Mar 21.—
1.

cer Hancock, Stratford, *Essex*,
eer; for an improved arrange-
and combination of certain me-
al means of propelling vessels
gh water. Mar. 21.—Sep. 21.

rt Brettel Bate, No 21, Poul-
ond., optician; for certain im-
ments. upon hydrometers and
rometers, for the term of seven
to be computed from the 21st of
inst., being an extension of
r letters Patent.

65. Francis Gybbon Spilsbury, Newman-street, Oxford-street, Engineer; for certain improvements on machinery or apparatus for stamping up and compressing metals or other substances. Mar 22.—Sep. 22.

66. William Maugham, Newport-street, Lambeth, *Surr.*, Chemist; for certain improvements in the production of chloride of lime, and certain other chemical substances. Mar. 22—Sep. 22.

67. William Hale, Greenwich, *Kent*, Civil-enginer; for certain improvements on machinery applicable to vessels propelled by steam or other power, which improvements or parts thereof are applicable to other useful purposes. Mar. 22.—Sep. 22.

68. William Westley Richards, Birmingham, *Warw.*, Gun-maker; for certain improvements in primers for discharging fire-arms by means of percussion. Mar. 22.—Sep. 22.

69. John Cox, Bristol, Soap-manufacturer; for certain improvements in the manufacture of soap, which will be particularly applicable to the fetting or fulling of woollen cloths. Mar. 22.—Sep. 22.

70. Sir John Scott Lillie, Fulham, *Middx.*, Knt., for an improved mode of acquiring power for the purpose of propelling carriages, barges, and other the like contrivances for conveying goods and passengers. Mar. 23.—Sep. 23.

71. John Lionel Hood, Newcastle-upon-Tyne, Gent., and Andrew Smith, Princes-street, Leicester-square, *Middx.*, Engineer; for an improved mode of manufacturing belts, bands, and straps, to be employed in place of ropes or chains, and for other useful purposes. Mar. 26.—Sep. 26.

72. William Blurton, Field Hall, near Uttoxeter, *Staff.*, Gent.; for an improved method of and apparatus for extracting milk from cows and other animals. Mar. 26.—Sep. 26.

ENROLMENTS of GRANTS in 1836.

am, Sharpening Tablet. Mar. 10.
h, Locomotive Steam-engine and Railway-carriage improvements. Mar. 23.

METEOROLOGICAL JOURNAL FOR FEBRUARY 1836; KEPT AT BLACKHEATH ROAD.

Day of Month	Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer Min.	Thermometer Max.	Daily Temp.	Solar Var.	Rad.	Clouds. A.M.	Clouds. P.M.	Wind. A.M.	Wind. P.M	Direction of wind A.M.	Direction of wind P.M.	Lunation.	WEATHER, &c.
Monday, 1	29·424	49°	29·420	50°	37°6	44°8	41°2	7·2	35°	2	2	3	3	W.S.W.	W.S.W.		Windy; very clear.
Tuesday, 2	29·075	49	28·801	5	33·4	40·2	36·8	6·8	30	10	10	1	1·2	S.	S.E.	○	Rain; unsteady wind from S. to E.
Wednes. 3	28·985	48	29·251	4	34·0	39·0	36·5	5·0	32	10	40	3	2	N.N.E.	N. N E.		Overcast with light showers; windy.
Thurs. 4	29·570	48	30·054	48	34·0	37·5	35·8	3·5	34	10	10	3	2	E.N.E.	E. N. E.		Do. small rain; dark throughout the day.
Friday, 5	30·300	47	30·290	48	35·9	37·9	36·9	2·0	35	10	10	2	1	N.N.E.	N.		Do. as before.
Satur. 6	30·122	46	30·100	50	33·6	47·9	40·8	14·3	30	10	4	1	1	W.	N.W.		Cloudy A.M.; afternoon mild with clouds broken.
SUN. 7	29·915	48	29·975	51	37·1	47·6	42·4	10·5	33	10	6	1	1	W.S.W.	N.W.N.		Showery till 11 A.M.; afternoon *cumuli*; even. rainy.
Mon. 8	30·200	50	30·061	51	33·6	46·0	39·8	12·4	31	5	10	1	2	S.W.	S.W.		Light rain at night and thick.
Tues. 9	30·051	51	30·080	52	44·0	52·1	48·1	8·1	43	10	5	2	2	W.S.W.	W. b S.		Much wind and *scud*.
Wed. 10	30·040	52	29·860	54	45·1	51·0	48·1	5·9	42	7	9	3	3	S.W.	W.S.W.	☾	Much cloud and wind; hail and rain at 7 P.M.
Thurs. 11	30·185	51	30·358	51	33·1	39·8	36·4	6·7	32	2	2	2	3·2	N.W.	W.N.W.		A snow & hail shower at 8 A.M.; *cumuli*; strong wind.
Friday, 12	30·250	50	30·202	50	29·7	46·0	37.9	16·3	27	8	1	3	2	W.S.W.	W.N.W.		Strong winds; mostly clear.
Satur. 13	30·515	48	30·501	49	27·5	40·6	34.0	13·1	23	1	0	1	1	S.S.W.	S.W.		Frosty at sunrise; very fine; cloudy night.
SUN. 14	30·551	50	30·568	51	36·2	49·5	42·8	13·3	31	1	5	1	1	S.W.	W.S.W.		Fine; *cumuli;* fine clear night.
Mon. 15	30·625	51	30·575	52	30·0	47·1	38·6	17·1	25	0	0	1	1	W.S.W.	W.		Cloudless; very fine and serene.
Tues. 16	30·481	50	30·284	51	28·1	47·0	37·6	18·9	23	1	10	1	3	S. b W.	S.W.		Cloudy and windy; rain in the eve.; snow fell at night.
Wed. 17	30·016	50	29·950	48	30·4	37·0	33·7	6·6	28	2	8	3	4·5	N.N.W.	N.N.W.	●	A heavy gale; light snow; tempestuous night
Thurs. 18	30·167	47	30·250	49	34·8	39·0	36·9	4·2	33	4	5	4	3	N.N.E.	N.E.		A heavy gale as before.
Friday, 19	30·400	43	30·398	44	30·0	35·5	32·7	5·5	29	5	7	2	2	N. b E.	N.		Clouds broken; very cold.
Satur. 20	30·501	44	30·525	46	24·1	36·0	30·0	11·9	22	3	0	1	1	N.E.	N.E.		Cloudy A.M.; perfectly clear.
SUN. 21	30·476	43	30·365	45	20·0	36·9	28·5	16·9	17	0	2	1	1	W.	W.S.W.		Hazy; sharp white frost; cloudy at night.
Mon. 22	30·112	44	30·001	47	32·2	44·0	38·1	11·8	29	8	9	1	1	W.	W.		Lowering; drops of rain; a cold hard sky.
Tues. 23	29·800	47	29·712	48	31·9	45·1	38·5	13·2	30	4	7	1	1	S.	S.S.W.		*Cirro-cum.* & *cum.-stratus;* fine clear night with rime.
Wed. 24	29·512	47	29·380	48	27·4	44·9	36·2	17·5	24	7	7	2	2	S.	S. b E.	☽	Much cloud; cold and raw; light rain at intervals.
Thurs. 25	29·196	48	29·178	49	29·1	40·0	35·5	10·9	26	1	1	0	1	S.	W.S.E.		Very fine; *cumuli;* clear evening; *cirrus;* cloud at [midnight.
Friday, 26	29·100	49	29·058	49	25·3	37·0	31·2	11·7	23	10	10	2	2	E.	E.N.E.		Rain and snow A.M.; rainy.
Satur. 27	29·028	47	29·134	48	32·6	36·4	34·5	3·8	32	10	10	1	1	N.W.	W.		Dark and cloudy.
SUN. 28	29·402	47	29·425	48	31·8	38·4	35·1	6·6	31	10	9	0	0	W.	N.W.		A remarkable dark day; a red mist or haze.
Mon. 29	29·600	48	29·651	48	30·5	39·0	34·7	8·5	30	10	10	0	0	N.	W.		Cloudy and thick.
Mean	29·909	48	29·897	49	32·72	42·11	37·08	10·05									

Bar. Max. 30·625 on the 15th. | Mean height at 9 A.M. 29·909 } M Press. || Ther. Max. 52°1 on the 9th. } Mean Tem. || Lowest point of Rad. 17°, on the 21st.
Bar. Min. 28·741 2d. | Mean do. at 3 P.M. 29·897 } 29·903 || Ther. Min. 20°0 21st. } 37°42. || Solar var. 10°05. Rain and snow fallen 1·83.

THE

MAGAZINE OF POPULAR SCIENCE,

AND

JOURNAL OF THE USEFUL ARTS.

HORÆ MAGNETICÆ.—TERRESTRIAL MAGNETISM*.

THE problem of determining the *number* and *position* of the magnetic poles of the earth has been a subject of repeated, but of uniformly unsuccessful, investigation: and, indeed, it is hardly yet agreed what property shall be considered a test by which the position of these poles shall be decided, or upon any character by which they shall be defined. Even the language in which the discussion is conducted is so vague, that it would be difficult to learn from it what is meant by the word "pole," and perhaps impossible to reconcile the statements we continually encounter, with any one definition of that point that either has been, or can be, given. There is, it is true, some notion of its being *a point* mixed up in all these discussions: but whether it be a centre of force from which the magnetic energy emanates—or a point towards which the free needle is always directed—or a point in which the presumed magnetic meridians intersect upon the surface of the earth—or a point at which the dipping needle becomes vertical—or a point at which, in reference to surrounding points on the earth's surface, the magnetic (directive) force has the greatest intensity—or the points to which the *Halleyan Lines* of equal variation tend—or a point about which the lines of equal inclination range themselves, as a focus—or, finally, a point to which all, or any number of these circumstances simultaneously belong—it would be altogether impossible to extract from the majority of writers on Terrestrial Magnetism. Should it, indeed, happen that all these definitions are virtually identical—that is, should all the rest result as consequences of the one which may be arbitrarily selected as the definition—then it is true that the reasonings would be concerning the same point: but yet it would still be, in the first place, incumbent on those who so used the term, to demonstrate that the assumed mathematical dependence was necessary, and, therefore, that the physical dependence was inevitable. This, however, has not been done. Nor has it, probably, been attempted, inasmuch as the investigation undertaken with that object, would have shown the error of the assumption itself; for it is remarkable that *not one of these properties determines (generally) the real centres of force, nor yet the position of the points possessing any one of the other properties above specified**.

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* It may safely be presumed that the leading phenomena of magnetism are familiarly known to most of our readers: but probably, few of those for whom our Journal is more especially designed, have looked very carefully into the history, or are at all aware of the present state of the science, and its several kindred branches. Nor are we aware of any work, in which the general reader can find satisfaction on any of them, except he be resolved to enter into a more extended course of inquiries, than are compatible, except with a life wholly devoted to scientific pursuits. Even of those who have entered upon a career of independent inquiry, few are fully aware of the efforts which have been made, the hypotheses which have been formed, the facts which are to be accounted for, or the fatal objections that have been urged against, even popular theories. To furnish the general reader with a familiar view of these, and the more extended inquirer with an analysis of the scarcer series of important papers, that have been published already in the transactions of different societies, in various periodical Journals, and in separate treatises devoted to those subjects, is considered by the conductors of this work as one of the legitimate and most useful objects of their Journal.

Instead, however, of taking up the different parts of this subject in the order required, in a formal and didactic treatise, the Editors have preferred to select only those which are of the most vital interest, and to arrange them according to their own particular views—a course, which, when fully developed, they are led to hope will be found in a good degree to combine utility with a clear exhibition of the system by which the whole are *actually* connected in nature.

It is not difficult to trace to their source, these mistaken opinions: nor is it without interest or instruction. The symmetrical form of the earth, and the analogy between its figure and the *terrellæ* which Gilbert employed in numerous of his experiments, together with the very natural hypothesis of its being itself a great magnet, led him to suppose that the centres of force were symmetrically situated on the earth as he generally found them in his *terrellæ*, and, therefore, at the extremities of the same diameter. There would then be an identity between several of the points above specified—as, for instance, those at which the needle would be vertical, at which the intensity would be greatest, round which the lines of equal dip would be ranged, to which the lines of equal variation all converged, and in which all the magnetic meridians intersected: but as the analogy between the earth and the iron ball could scarcely be supposed to be a very intimate one, except as to general geometrical figure, and the possession of the magnetic properties, the assumption, though very natural at first sight, ought to have been rejected, as well as the whole train of consequences that flowed from it, as the extreme improbability of the conditions existing in the two bodies that would render the analogy possible, became perceptible. If, indeed, similarity of geometrical form could contribute to the disposition of the poles, there could not be any reason for the existence of poles on one part of the spherical surface rather than another, and hence, so far as poles were peculiar points, distinguished from those which were contiguous, there could not possibly exist such upon the sphere. If, on the contrary, they depended upon the material of which the *terrellæ* were composed, the capability which that material possessed of having its symmetry of figure destroyed, without losing its magnetism, would at once offer conviction that there was no *necessity* for the points in which this principle was especially apparent, being symmetrically situated upon symmetrically figured bodies. The fact, however, that an iron ball of tolerably uniform structure, generally did become magnetic, so that its poles were in a diameter, furnished an analogy that was considered sufficient to justify the assumption of the magnetic poles of the earth being symmetrical: even though, had the uniformity of its composition been the cause, there was not the slightest reason to think that the earth did possess that homogeneity of structure that would be essential to the symmetrical developement or disposition of the magnetic energy. The *poles were hence* considered to be *two*, and situated either at the actual

* Of some *recent* researches on this subject we shall speak hereafter.

extremities of the terrestrial magnet, or so near it, as to be productive of effects not appreciably different from those which two superficially situated poles would produce.

The researches of Halley in respect to the actual directions of the *horizontal* needle, compared with those which on this hypothesis they should have taken, showed him the utter incompatibility of the two classes of results, and hence the fallacy of the hypothesis itself[*]. Nevertheless, though that opinion was thereby exploded, it has left so many of its erroneous consequences mingled up in our language and fundamental dogmas, that even now, so long after the opinion has become a mere matter of scientific history, the consequences of it seem to be almost universally admitted even by philosophers themselves! Thus forming one more, in addition to the innumerable pre-existing instances, of the influence of language upon the progress, or rather in arresting the progress of scientific research and discovery!

The hypothesis of two poles in the same diameter of the earth, but not equi-distant from the centre, could not, of course, offer itself to those who considered the poles to be either actually or approximately superficial: nor that of two poles, either on the surface or below it, but not diametrically opposite, to those who considered that magnetism was, of necessity, symmetrically developed in a body of geometrical symmetry of figure. To Halley, therefore, no resource remained, but to try how far the observed variations of the horizontal needle could be made to agree with the hypothesis of *four* poles, two and two symmetrically situated, or nearly so[†]. He first assumed them fixed: but this giving no account of the change of declination that was continually taking place, he supposed two of them changeable in position, and the other two permanently fixed. The mechanism, indeed, which he has proposed for effecting this continual change, is sufficiently difficult to admit, and has been the chief stumbling-block to the reception of the theory of four poles. However, what estimate soever may be formed of his mechanism of the internal globe, in which one of his magnets was placed, moving with a different angular velocity from the external shell in which the other magnet was situate, there is no doubt, but the idea of four poles was formed in the true spirit of the inductive philosophy, and that it strikingly marks the high mental character of its illustrious author. Halley proposed it as a conjecture, for the purpose of explaining the phenomena, which he believed could not be deduced as a consequence of the action of two poles merely: yet, he entertained but little hopes of finding any hypothesis that should satisfactorily account for all the phenomena, and hence he urges the necessity of determining the actual details by means of actual observations. He merely conceived, that the hypothesis of four poles furnished a better approximation to the *general features* of the observed lines of equal variation, than the hypothesis of two superficial poles that had before been considered the adequate cause of the phenomenon. His hypothesis, for instance, required that there should at different points of the same terrestrial meridian be both easterly and westerly variation, and thus agreed in the observation: the other hypothesis of two poles diametrically opposite, required that only one kind of variation should be found on the same meridian, which was altogether incompatible with observation.

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[*] The hypothesis required the variation from the same meridian to be always of the same name; but observation showed that on all meridians there were variations of both names, and therefore one or more points at which there was no variation at all.—*Phil. Trans.*, 1703.

[†] The idea of an *odd* number of poles had not perhaps then been entertained, or even suggested, and hence could not have been tried; or even, if tried, would hardly help him in his difficulty. It would be much more natural to suppose two magnets in the north, each having two poles, than to suppose there was one magnet with three poles, or two magnets, one of which had only one pole; I say more natural, simply because more in *uniformity with* the analogous cases which artificial magnets daily *exhibited.*

The lines laid down by Halley, were in several places faulty, inasmuch as they were furnished from voyages made long before the epoch for which his celebrated chart was constructed (1700), and no allowance had been made for change of declination during the intervening periods, since its direction and amount could not then be ascertained. The change in the positions, also, even of those which he had laid down correctly, of course, speedily rendered his chart useless for the purposes of navigation—to the facilitating of which his labours were directed—and hence new series of observations were collected and collated, greater in number, and more accurately made: and the result was, a new chart of the *Halleyan Lines*, published in 1744*, and formed from a discussion, it is said, of more than 50,000 observations, by Mountaine and Dodson. These lines assumed, in some respects, a less *bizarre* appearance than those in Halley's own chart; and probably, it was to the comparison made upon the two charts, that we owe the two extremely ingenious, but still unsatisfactory, papers which the great Euler has published in the Berlin *Mémoires* for 1757 and 1766†.

Euler, as appears from several other papers of his‡, adopted the doctrine of the magnetic *current*, which moved the needle by impulsion; so that when the needle came to a quiescent state it was a tangent to the curve in which the fluid moved§. The laws of force in this curve were unknown, and even conjecture had been sparingly hazarded as to the geometrical nature of the trajectory of this perplexing ethereal stream; and hence, it was impossible for Euler to take up the problem as a physical one, but was compelled to consider it as a descriptive one, the law of description itself being purely conjectural. It might, indeed, at first sight, seem that little, if anything could be gained by such a change, and that the description would even be more distantly removed from the true theory, than the physical conjecture would be. A moment's consideration, however, will correct this notion.

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* And again in 1756. Mr. Barlow has also laid down a new set of lines, from observations more recently obtained, and more carefully made: but we shall speak of this hereafter.

† No account, so far as we know, has been given of the method employed by Euler in these papers, in any English or foreign work (except Hanstein's *Magnetismus der Erde*,) on terrestrial magnetism; nor are we aware that in any work in our own language there is even a reference made to them, except a very casual one by Professor Robison, in the article "Variation," in the *Encyclopædia Britannica*. We hope, therefore, that we are rendering a service to men of science by giving in this place the analysis which we have made of those papers, in the briefest and most popular form we can devise. For most purposes, this general view will be sufficient; and where this is not the case, the consultation of the papers themselves will be rendered easier by this exposition of their contents.

‡ *Mém. de l'Acad. des Sciences, Prix*, tom. v.; *Opuscula Analytica*, tom. iii., *p. 1—53, etc.*

§ *The* experiments and reasonings of Lambert first showed the fallacy of this *opinion: vide Mém. de l'Acad. de Berlin*, 1766, p. 22—48.

The geometrical effects of a physical law, so far as trajectory is concerned, are almost always encumbered with branches, which render the hypothesis from which they flow (where it does not happen to be the true one) obviously fallacious and inapplicable: but when the figure of a trajectory, is altogether known from observation, the knowledge which most geometers possess, of certain of the simpler curves, enables them to select such of them, as bear at least an approximate resemblance to the trajectory of observation. It was thus, that Kepler was led to the elliptic forms of the planetary orbits: it was thus, too, that Sir John Herschel was led to consider the lemniscata (or ellipse of Cassini) as the figure of the isochromatic curves. One of these conjectures was subsequently verified as the necessary result of a physical law, when the body was undisturbed by foreign influences: the latter remains still unjustified, save by a comparison with experiment. Other approximate results as to trajectory, resulting position of bodies acted on, and in all the varieties of the geometry of physics, might be quoted, some of which have been verified, others remain still undecided, but the great majority of which have been subsequently exploded in consequence of the application of effectual tests giving results incompatible with them. This circumstance is indeed of precisely the same nature as the construction of empirical formulæ, in respect to the quantitative relations among the objects of physical research generally. They have their use: and great skill is often required to frame these descriptive or empirical hypotheses so as to answer the more immediate purposes, which a knowledge of the true law of physical operation is fitted to do. Simplicity of construction, and rapid numerical approximation, are their chief requisites. In this respect, Euler manifested his extraordinary sagacity, not less than in any of his other researches: and it must be confessed, that the present is amongst those very happy conjectures which must ever be remarkable as an empirical construction for giving the *quantities* sought, without even approaching, in the slightest degree, in the algebraical form of its results, to the algebraical form of the result of the true theory. Though he did not hit upon the true law of description, yet, he certainly formed one which gave results, differing in many cases extremely little, and in no case differing very widely, from the phenomena they were intended to represent. His system, too, was one of great geometrical and algebraical simplicity. In the second paper, he slightly modified the method he employed in the first: but in some of the cases, it is certainly more remote from agreeing with the phenomena, than the original hypothesis was.

Dissatisfied with the argument of Halley, (that the result of two poles would be, the same kind of declination on the whole length of any given meridian,) and actuated, too, by the great difficulty of investigating the case of four or more poles, Euler resolved to try whether the poles having other than diametrically opposite positions, would not account, at least *approximately, for the* variations figured in the charts of Halley, and of Mountaine and Dodson. He would thus, in case of tolerable success, effectually destroy the force of Halley's chief objection to the duality of the poles, by showing that two, when properly assumed, would give the declination under the same meridian partly east and partly west. "Comme je prouverai cela indubitablement dans la suite," he adds, in conclusion, "il me sera permis de regarder l'hypothèse de quatre poles magnétiques comme forte douteuse; et avant qu'on ait très évidemment prouvé, que deux poles magnétiques ne sont pas suffisans pour expliquer les phénomènes de la déclinaison magnétique, ce seroit contre les régles d'une bonne physique si on vouloit recourir à quatre poles."*

Euler's researches were entirely confined to the horizontal needle, and his hypothesis is simply this:—*that the horizontal needle is a tangent to the circle passing through the place of observation and through the two points on the earth's surface at which the dipping needle becomes vertical, or at which the horizontal needle loses all directive tendency.* At art. 55, of his first paper†, he thus expresses his principle, and his reasons for adopting it. "La solution de ce problème et des suivans est fondée sur ce principe, que la direction magnétique sur la terre suit toujours le petit cercle, qui passe par le lieu proposé et les deux poles magnétiques de la terre. On m'accordera bien ce principe à l'égard de la véritable direction magnétique, qui renferme ensemble l'inclinaison et la déclinaison; mais puis que la déclinaison dont il s'agit ici, se régle sur le plan vertical, qui passe par la direction magnétique, on en pourroit tirer quantité d'objections, dont la discussion menerait trop loin, et surpasseroit les bornes de notre connoissance. Mais on pourra en sorte fixer les idées, qui entrent ici en considération, que ces objections n'y ayent plus de prise. Si l'on plaçoit là les poles magnétiques de la terre, où l'axe magnétique traverse la surface de la terre, on seroit sans doute fort embarrassé; puis que la déclinaison n'y seroit plus indéterminée, à moins que l'arc magnétique ne passeroit par le centre de la terre. Par cette raison j'établirai les poles magnétiques de la terre là, où la véritable direction magnétique est verticale, de sorte que dans ces endroits il ne puisse y avoir question de la déclinaison; et ce sera dans ces points, où toutes les lignes Halleyennes doivent aboutir, de même qu'aux poles naturels de la terre. Or déterminant en sorte les poles magnétiques de la terre, sans s'embarrasser de l'arc véritable magnétique, les objections mentionnées n'empêcheront plus, qu'on n'accorde le principe établie, c'est à dire que partout l'aiguille aimantée dirige suivant la tangente du petit cercle tiré sur la surface de la terre par chaque lieu proposé, et les dits poles magnétiques, où l'inclination devient verticale."

Euler divided his paper into four general cases:—

1. Where the poles are diametrically opposite:
2. Where they are not diametrically opposite, but still in opposite meridians:
3. Where they are on the same meridian:
4. Where they are anyhow situated.

The first then he finds insufficient to furnish any approximation to the phenomena of terrestrial magnetism: though he finds in all, except *the first*, an answer to Halley's fundamental objection to the duality of the poles*. Now, that the law of force is known, it is easy to see that the projection of the dipping needle, upon the horizontal plane, cannot coincide with the tangent to a circle drawn through any two fixed points: and hence to ascertain that the descriptive hypothesis of Euler is incompatible with the necessary consequences of the true theory of magnetism. Euler, however, from not having any method by which, from observed positions of the needle, to determine the positions of the two points which he has called poles, was unable to do more than approach towards them by successive tentative operations. After all his attempts he found discrepancies: but these he attributed not so much to any error in his hypothesis, as in the difficulty of fixing the position of the poles, and the imperfection or inaccuracy of recorded observations. Had it occurred to him to seek the equation of condition that must subsist amongst the constants in the equations of their circles, that they may have common points of intersection, it is obvious that three good observations would have enabled him to calculate the actual positions of the corresponding magnetic poles: and hence he would at once have been able to bring his hypothesis to a direct and decisive test†.

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* *Mém. de Berl.*, tom. xiii., p. 177.

† Ib., p. 200—1.

So long as the poles had been considered to be only two, and diametrically opposite, the point at which the dipping needle would become vertical, and those towards which the Halleyan Lines ultimately tended, were points identical with those in which the axis of the magnet pierced the terrestrial surface. Euler's rejection of that symmetry of the distribution of magnetism led also to a separation of the systems of points, each of which had alike before answered to any definition of a pole. Some authors have subsequently fixed their attention upon one definition, and others on another, under almost every variety of aspect: yet still they all alike call the points defined in their respective ways by the common epithet, "pole." Much vagueness and confusion have arisen from this; and more especially as, for the most part, the term has been used without any previous definition at all, and the import of it left to be derived from the manner in which it is employed in the investigation. It is even, by many writers, used in more senses than one,—that is, to designate points which are essentially distinct, except in the particular case of the diametral situation of the points in question. Some respectable authors have thereby not only led their readers into difficulty and error, but have even been led into singular mistakes on their own reasoning, in consequence of their mistaken condensation of two points essentially distinct, into one single point. Euler, however, very properly discriminates here between the points at which the needle is to be vertical, and those at which the magnetic axis intersects the surface of the earth: and from the first and well-established principles of magnetism, he is right in considering the point at which the horizontal needle loses its directive qualities, and those at which the dipping needle becomes vertical, as identical points. The contemplation of the magnetic curves (or currents), too, enabled him to see that if the needle ever could become vertical to the earth's surface, whilst the magnetic energy was unsymmetrically distributed with respect to the geometrical figure of the earth, the centres from which these currents emanated must lie within the earth, and at some considerable distance from the surface. Euler views this as a matter of course, not necessary to dwell upon, but either so familiar to his own mind, or perhaps to other philosophers also, as not to require to be enforced by a single reason, such as we should expect would be the case with a novel or unobvious doctrine. If, however, we except Halley's poles of his Terrella, which may be viewed as fixed deep below the surface of the outer shell of the earth, this is the first distinct view that we have met with of that consequence respecting the positions of the centres of magnetic force. The definition of the pole which is employed by Euler, is that which has most commonly prevailed since his time; but the methods of determining it, and its connexion with other points, from observation, have been mixed up with errors of various kinds, and errors too, in principle as well as in detail.

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* That is, with respect to the *present* state of the Halleyan Lines; but he thinks, that from the motion of these poles with respect to each other, each case might possibly be applicable to some past or future state of terrestrial magnetism.

† For the determination of such points, under such circmmstances, see our Note A. *where the requisite equation* of condition is investigated.

Subsequent researches led Euler to discover that his assumption respecting the needle being a tangent to the forementioned circle, was erroneous, and incapable of being made to accord with the phenomena completely. He, therefore, substituted the following, under the title of *Corrections necessaires pour la Théorie de la Déclinaison Magnétique proposée dans le* xiii. *volume des Mémoires.*

He supposes now that the true magnetic poles are at *the surface of the earth**, the chord joining which he calls the magnetic axis, and its middle the magnetic centre. Then a line drawn from the place of observation to the magnetic centre, being made to form the base of an isosceles triangle, one side of which being coincident with the magnetic axis, the other side will be the line of rest of the freely suspended or dipping needle. This hypothesis (and he actually calls it such; vid. vol. xxii., p. 227) fulfils certain conditions that were essential to any good theory. 1. It gives the needle the approximately accurate positions at the equator, the needle and axis being then parallel. 2. It fulfils the condition of the needle and axis forming a continuous line when at the poles. 3. It furnished two points at which the needle would be vertical. 4. It gave a series of positions, single for each place, and having a certain, and oftentimes pretty close, approach to the true position. Its defects are:—

1. That it is inconsistent with the *since discovered* laws of magnetic action; but this, of course, ought not to be urged strongly here, since it only invalidates the hypothesis itself generally, and does not point out its peculiar discrepancies with phenomena.

2, That in all experiments with magnets we uniformly find the position of verticity to the magnetic axis (much more than of verticity to the circles, whose common chord that axis is), *beyond* the extremities of the magnet itself: whilst Euler's hypothesis furnishes positions *between* the extremities of the magnet. Vid. pr. vi., p. 233.

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* It is extremely singular, after once entertaining the views he seems to have done in his former paper, as to the internal position of the poles, that he should have adopted this opinion of their superficial position. Possibly, indeed, he might not have *intended* to express any view at all on that subject in his former paper, and we might *have interpreted him*, therefore too liberally.

3. That it gives values of the inclination extremely remote from those furnished by observation.

4. That it does not furnish a magnetic equator differing from a great circle, and, consequently, in this respect, too, it is incompatible with observation. This, however, with one exception, is common to all the theories that have been proposed as to the position of the magnetic poles, ever since the true law of the variation of magnetic force has been accurately determined and generally understood.

5. That a curve, described by the continued intersections of the needle, according to his hypothesis, in any one plane passing through the magnetic axis, bears but little resemblance, as to actual form and curvature, at corresponding values of the abscissa, to the true magnetic curve.

By means of some lemmas in spherical trigonometry and this hypothesis, he proceeds to finds expressions, successively, for—the inclination of the needle, as it is referred to a tangent at the place of observation to the magnetic plane*—the inclination of the needle to the horizon—the declination of the needle from the geographical meridian—and one or two other lines and angles. These expressions are derived by a mixture of plain and spherical trigonometry, and his preceding lemmas (to which no exception can be taken, except that they are much less elegant and systematic than Euler's mathematical processes commonly are): and they are, for the most part, exceedingly complicated in their forms. He does not attempt to discuss the form of the Halleyan Lines under this hypothesis; and we doubt whether it would be possible to obtain an equation which would express them by means of these expressions and methods: but he proposes to substitute another class of lines, which he calls "magnetic routes," instead of them (p. 244), which he thus defines—*Les routes magnétiques sont des lignes tirées sur la surface de la terre, dont des tangents marquent en chaque lieu le direction de la boussole*—and finds them to be less circles of the sphere whose centres are in the magnetic equator, and whose circumferences pass through the poles where the needle is vertical.

In a suggestion at the close, he purposes to consider the magnetic centre slightly removed from the middle of the magnetic axis, to make it agree with any slight discrepancy between the result of his hypothesis and the phenomena furnished by observation: but seeing that this circumstance would render the calculation much more difficult, he does not attempt to give, or even to suggest any details respecting the effect of this last named modification. From the complexity of the results already laid down, there could be little hope that the subject under this new aspect could be effectually treated in any one of its branches by means of the mathematical processes which he has laid down, or by others having any direct analogy to them.

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* He calls them, very properly, magnetic meridians. They are the planes, passing through the true poles, and the several places of observation. In the language of the present day, the term, magnetic meridian, is exceedingly inappropriately used. By this term is commonly meant, the vertical great circle, passing through the axis of the needle. Various peculiar phenomena, have been imagined to take place in this plane, and it is a favourite position amongst meteorologists, in which to discover peculiar modifications of the Aurora Borealis! It would be difficult to assign any reason, admitting the connexion between the causes of terrestrial magnetism, and the aurora, why a plane *not* passing through the centres of magnetic force, should be expected to have any peculiar claim to these remarkable peculiarities of phenomena, in preference to any other. The truth is, these peculiarities take place in every possible position; but, like the predictions of future events, rainy days, &c., when a dogma is once laid down the fulfilments are faithfully recorded, and duly dwelt upon, and as even rain a *day or two before or after* fulfils the wizard's prediction, so the phenomenon having an azimuth a few degrees east or west of this magnetic meridian, is easily by a little imagination, or a little fashionable philosophical "coaxing," tranferred to the plane itself:—the *more extreme cases of* exception being wisely forgotten!

Meanwhile, attempts to discover the true law of magnetic action were gradually working towards a decided proof of that law which is now established beyond all question—the inverse square of the distance. It appears to have been first distinctly proved by our countryman Michel, and published in his *Treatise on Artificial Magnets*, in 1750. He had the merit, too, of introducing the use of the Torsion Balance into the method of experimenting on the law of action of such forces—the instrument by which Coulomb, with singular address, fully and satisfactorily established the law. Lambert, however, in the volume (xxii.) to which we have referred for Euler's second paper, published a dissertation on the same subject. This paper displays that address both as a mathematician and an experimenter, for which Lambert was celebrated in a high degree: but the *method* involved difficulties and fortuitous disturbances against which it was impossible to oppose the most consummate skill and discrimination. He, by a very ingenious and subtle mode of reasoning, established the true law of magnetic action, and showed a considerable approximation between its results, and the results of those experiments which he made*. They were repeated and varied by Dr. Robison, both as to circumstance and the form of the apparatus: and that truly eminent philosopher was led to results still more closely agreeing with theory than Lambert had obtained. Notwithstanding this, as he still thought the discrepancies were more than *ought* to have existed, he was too candid to urge that the law was established beyond the possibility of dispute. (Supplement to the third edition of the *Encyclopædia Britannica*, vol. ii.; and *Mechanical Philosophy*, vol. iv., p. 340.) He stood alone, however, in his estimate of the proof afforded by the experiments of Coulomb; and forms the solitary instance of doubt respecting the conclusiveness of that completely decisive course of experiment in finally deciding the question. Besides the memoirs of Coulomb himself, in the *Mém. de l'Acad.*, the best view of them is given by Biot, in the third volume of his *Traité de Physique*, p. 66—70.

Though the magnetic dip had been very early discovered, yet it seems to have been uniformly neglected in discussions respecting terrestrial magnetism, with a few such exceptions as Euler, and Lambert, and Mayer. The variation seems to have been thought to be *total* effect of the cause, whatever that cause might be which produced it. The intensity of the magnetism, as manifested by the horizontal needle, was, indeed, observed with attention as to its daily changes, by Graham, nearly a century ago; yet no particular attention was paid to the changes it underwent in passing from one place to another, till nearly the close of the last century: and though it was suspected that the intensity like the dip increased from the equator towards the poles, yet till the return of Humboldt from his South American expedition, and the discussions of the observations which he had recorded by Biot, no certain evidence that such was the case, existed. Biot appears to have been the first who attempted to apply the laws of the variation of magnetic force to the actual determination of the magnetic poles, which he did in a *mémoire* read before the National Institute, at the end of 1804[*]. Laplace also gave the formula which connects the intensity in two different planes with the actual intensity of the force, and actual dip of the needle: and as Humboldt had observed all these, they operated as mutual checks upon one another, and verifications of the general fidelity of the recorded observations. This is the more satisfactory, as Biot has remarked, inasmuch as Humboldt could not be warped in his judgment by any hypothesis of his own, since he could not possibly foresee that Laplace would submit the observations to so severe a test.

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* *His determination* of the position of repose of a needle, subjected to the influence *of a magnet, will* be considered hereafter.

Biot, however, in his investigations, was obliged, as his predecessors had been, to confine his attention to a representation of one of these essentially-connected phenomena, that is of the intensity, dip, and variation, and it was the dip which he selected for the purpose. "In regard to the declination and intensity," he says, "we freely confess that we are entirely unacquainted with their laws, or their causes; and if any philosopher is so fortunate as to bring them to one principle, which explains at the same time, the variation of the inclination, it will no doubt be one of the greatest discoveries ever made." He does not even consider his "hypothesis as anything real, but only as a mathematical abstraction useful to connect the results, and proper to ascertain in future whether any changes take place." Still he does not view his formulæ as an "empirical construction of the observations." Probably, he intends by empirical formulæ, formulæ which are fortuitously taken, and so modified by means of the constants, as to agree in their results with observation: yet still, if his physical hypothesis is not to be considered "anything real, but merely as a mathematical abstraction useful to connect the results," there does not appear to us to be any *essential* difference between it and an empirical formula, since it is confessed to be only the *algebraical expression of an empirical law*—of a law which lays no pretension to physical accuracy. Yet it is upon the evidence of an investigation, thus characterized by its author, that the position of the poles, as concentrated in a molecule at the centre of the earth, has been generally adopted, and upon which the opinion maintains its ground to the present day, there being no new evidence nor new argument produced in favour of it.

Biot's hypothesis is—that the magnetic forces are situated in two points (or, at all events, that the resultant of all the forces of each separate kind, is a single force situated in a fixed point)—that these forces are equal in intensity but contrary in their quality—that these forces vary inversely as the square of the distance of the magnet acted upon—that these centres of force are situated in the same diameter of the earth—and, lastly, that they are equally distant from that centre.

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[*] This memoir has been translated in the 22nd volume of the *Philosophical Magazine*, and considerable extracts from it (all the essential parts) have been inserted in the article "Magnetism," in the later editions of the Encyclopædia Britannica. *It was originally published in the Journal de Physique*, for the year 13 (1804-5).

Upon the first* of these hypotheses, considerable diversity of opinion has prevailed, and under every aspect the decision of the question is clogged with difficulties, both experimental and mathematical; but if this were admitted, it would seem almost inevitable to admit the second of the hypotheses. The third is now undoubted by all philosophers; but the fourth and fifth are not only unnecessary restrictions,—they are also incompatible with other phenomena, which must be accounted for by the true theory, whatever it may prove to be. The magnetic equator, for instance, must be a great circle—which it is not; the parallels of magnetic latitude ought to be actually parallel and circular—which they are not: the variation of the horizontal needle ought to be constantly of the same name (E. or W.), through the whole extent of any given meridian—which it is not; the magnetic intensity ought to be altogether free from sensible change—which it is not. The hypothesis, even when the poles are at unequal distances from the centre of the earth, and at finite distances, is still liable to nearly all these objections, the form of statement of one or two of them being slightly modified. For instance, the magnetic equator would cease to be a great circle, but it would still be a circle—which is contrary to experience: the lines of equal dip would still be parallel, though at different distances in the two hemispheres—which is contrary to experience; the magnetic intensity would now become (probably) sensibly different, but the lines of equal intensity would, like the lines of equal dip, be parallel to the magnetic equator—which is contrary to experience; and, finally, the objection unmodified, arising out of the necessary magnetic variation, still retains its fatal power against the hypothesis under this view also. Nor are these all the difficulties of the hypothesis; but as any one of them is sufficient to overturn it, we need not here enumerate more than are already set down.

In this dissertation, however, Biot not only pointed out the true mode of investigation, but derived also the formulæ for the dip in terms of the magnetic latitude, as well as determined the approximate position of the magnetic equator itself (viewed as a great circle), which are admitted (with slight modifications of the formulæ, and slight changes of position in the equator,) to the present time. The modified formula was furnished from original and independent investigation by Krafft of

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* In his discussion of the state of the magnetic forces in a saturated bar, (in his *Traité de Physique*, vol. III., p. 77,) Biot has adopted a different hypothesis; viz. that at any point, the quantity of feromagnetism, is expressed by $\mu^x - \mu^{2l-x}$, where μ is a constant, depending on the state of the magnetised body generally, $2\,l$ is the length of the bar, and, x the distance from the extremity of the bar, which contains one specified kind of magnetism.

Would not Biot's formula be improved, by referring the origin of x to the middle of the bar? The result would be $(\mu^{l+x} - \mu^{l-x})$. This would be more elegant in a geometrical point of view at least; but the writer of this paper will have occasion, *shortly, to show,* in another place, the fallacy of the whole reasoning; and hence it is *unnecessary to dwell* upon it here with any greater detail.

Petersburgh, and the determination of the change in the position of the equator, arising out of its positions at two different epochs, was effected by M. Morlet*. The method of Morlet consisted in the determination of certain points, in or very near the equator, from actual observation, and then from other points more remote, interpolating by means of Krafft's formulæ—the latitude being estimated on great circles, passing through the axis of the magnetic molecule, and the place of observation. We shall see hereafter the *degree* of inaccuracy that must result from this process; or rather, we should say, from the theory on which this process is formed. We have noticed above some fatal objections to the theory itself.

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* *Mém. des Savans Etrang.*, tom. iv.

[To be continued.]

THE VILLAGE OF LEADHILLS, LANARKSHIRE.

(From a Correspondent.)

The village of Leadhills is situated in the parish of Crawford, and county of Lanark, Scotland, at the altitude of 1280 feet above the level of the sea. The prevailing rock in the neighbourhood is Greywacké, but at no great distance clay-state and greenstone are found, and coal within ten miles at Sanquhar and Douglas. The altitude of the Lowthers, the highest hill in the neighbourhood, as taken geometrically by Sir John Leslie, is 2396 feet; but, according to my measurement, made with a theodolite, under very favourable circumstances, its height is 2409,—or 13 feet higher—a difference scarcely appreciable upon such an altitude.

According to common report, the lead-mines were discovered by a German of the name of Bulmer, when searching for gold in the banks of the adjoining rivulets. This account is extremely probable, for the numerous hillocks on the banks of the streams which discharge themselves into the Clyde and Nith, bear evident indications of having been thoroughly searched for that precious metal: even so much so, that those miners, who at present amuse themselves, during their leisure hours, in searching for gold, cannot find a spot that has not previously been explored.

The method of searching for gold is, I believe, the same in every country; however, the one adopted at Leadhills is as follows:—The surface of the rock is laid bare; the earth, sand, &c., in its crevices are collected, riddled, and washed. The water carries away the earthy particles, and leaves those materials which are of greater specific gravity than itself, as flint, quartz, and what gold there may be. The proceeds from one puddle, as it is called, are generally, at the most, not more than a few particles, not larger than the point of a pin; but a man, in six hours, at an average, may collect about 4*d*. worth of gold. Some pieces, however, have been found as large as a pea, or small bean; and in 1827, one of the overseers at Wanlockhead had a piece so large imbedded on quartz.

These gold mines, however, were once productive, for history informs us that in the reign of James the Fifth of Scotland, 300 men were employed in them; and when that monarch, in a hunting-excursion in the adjacent moors, dined in Crawford Castle: each of his retainers, for dessert after dinner, were presented, on a wooden platter, with a few bonnets crowns, as the produce of the soil. These pieces were coined from gold, obtained in the mines of Glengonar, the rivulet upon which Leadhills is now built.

Lead, however, for centuries has formed the mineral riches of these mines. The vein has more than once expanded to the enormous width of 14 feet, but, I myself never saw it more than 4½ feet, and this was considered by the miners as a *very good* lead. At present the crip or annual number of bars (of 144lbs. each) amounts to 10,000 in Leadhills, and to about 8,000 in Wanlockhead, whereas at one period 35,000 were made at the former, and 15,000 at the latter.

It is not, however, to facts such as I have mentioned, that I particularly wish to call your attention, but to the mental superiority of these miners, over miners in other parts of the world, and to show, that even among a class of workmen, who might be supposed incapable of profiting by good example; one man of intelligence, may produce beneficial effects, that for ages will be felt and duly appreciated. The individual to whom I refer, was Mr. James Stirling, who, in the middle of the last century, was overseer at Leadhills. Mr. Stirling is known to the mathematical world, for two elegant propositions which he communicated to the Royal Society of London in 1735, for determining the form of a homogeneous spheroid turning round its axis; and which when applied to the earth, perfectly coincided with Newton's determination, that the revolving body was not an accurate elliptical spheroid, but approached infinitely near to that figure. When at Leadhills, he instituted a library among the miners, and strongly advised them to subscribe; and with such success, that there is not a workman about the village who is not a member of the library. The number of volumes, embracing the standard works on every branch of science, in 1830, amounted to two thousand, whilst the miners at Wanlockhead, another mining village within two miles of Leadhills, but in the county of Dumfries, possess another library, almost equally extensive. The effects of such institutions have been felt, not only in civilizing the inhabitants generally, but the small village of Leadhills, containing 1200 inhabitants, has the honour of producing two men whose names bid fair for immortality, Allan Ramsay, the poet, and William Symington, the engineer. Captain Basil Hall, in his *North America*, states that Symington was the first to apply steam to the propulsion of ships, at least in America. It is true, that Symington was one of the claimants, but the invention of steam-navigation, is one of those points of dispute, which probably will never be settled. We are, however, certain that Ramsay was the father of modern Scottish poetry, the precursor of Ferguson, of Burns, and of Scott.

Whilst in other mining districts, crimes are of frequent occurrence; none excepting petty offences were ever committed here, and whilst the *children of colliers* and miners are generally entirely illiterate, there is *neither a boy nor* a girl in these villages who cannot read, and most of em can write. In 1832, I visited these villages, with some friends of ine from Edinburgh. Keen phrenologists, who of course attributed the ental superiority of these miners to cranial developement, but to what-er cause it may be attributed, and I think it is due to the taste for ading that has been produced among them, the apparent comfort of the ople—the neatness of dress of the children, and the intelligence of the en cannot be denied. For not only to miners in others parts of the orld, are they superior, but to the working classes even in Scotland, hich is admitted to possess the most intelligent peasantry on earth.

Seeing, then, that such beneficial effects have been produced by such pparently small causes, might not the overseers of other mining districts, stigated by the example of Leadhills, try to institute Reading Societies mong their workmen? for it will always be found, that correctness of oral conduct follows the cultivation and enlightenment of the mind. is true that the Leadhills and Wanlockhead miners possess two special lvantages, they only work 6 hours in the 24, and have the perquisite of btaining as much land from their landlords, the Earl of Hopetoun at eadhills, and the Duke of Buccleuch at Wanlockhead, as they can culti-ate with the spade. The last might be considered as of hardly any lvantage in a pecuniary point of view, as the uncultivated land in the eighbourhood rents at 2*s.* per acre, but it is still unknown what spade bour can effect, even in the most unpromising circumstances, as will be roved from an account which I have received of the enormous crops that ave been produced at Leadhills, and which I communicate as being nportant, not only in an economical, but also in a geological, point of iew.

The altitude of Leadhills above the level of the sea, is, as I have lready mentioned, 1280 feet, and to show the mean temperature of eadhills, and the quantity of rain that fell there during the summer of 828, I make the following extract from a paper of mine, which appeared Jameson's *Edinburgh New Philosophical Journal.*

The temperature was taken at 7½ A. M., and 8½ P. M. The daily mean as then taken, and again the monthly.

The height of the gauge above the sea was 134°, and it was placed ear to the highest inhabited house in this island.

May,	$49\frac{2}{3}$°			
June,	54_{14}°	Rain which fell	$3\frac{4}{5}$	inches.
July,	$55\frac{1}{2}$°	- - -	$8\frac{7}{20}$	..
August,	56°	- - -	7	..
Septem.	$49\frac{1}{4}$°	- - -	7	..
October,	45°	- - -	9	..
			$35\frac{3}{20}$	inches total; an enormous quantity.

'he account to which I refer appeared in the *Scotsman*, and is also copied ato Colburn's *New Monthly Magazine* for March. It is as follows:— Mr. John Hunter, Leadhills, planted, in 1835, 16 Scotch falls (being he tenth part of a Scotch acre) with potatoes, which produced the extra-rdinary quantity of 335 imperial stones, being at the rate of 21 tons to he Scotch acre" (or 17½ to the English). And from a square mile of *urface, around the village*, it is calculated that 25,000 stones of hay

(22 lbs. to the stone) and 12,000 stones of potatoes are annually produced." The allotment system has been strongly advocated by Mr. Howitt, in Tait's *Edinburgh Magazine*, as being adopted to a considerable extent, and with great advantage, both economically and morally, as confirming the moral sentiments and social condition of the people in the neighbourhood of Nottingham; but the climate of Nottingham, is, I believe, as fine as any in England, whereas, at Leadhills, it is quite the reverse; and still human labour has triumphed over the sterility of the soil, and the backwardness of the climate.

"When further attention," remarks the editor of the *Scotsman*, "is now so generally called to the practicability of improving our waste lands, this instance of productiveness at Leadhills, a mountain-district, higher than the summit of the Pentland range near Edinburgh, ought certainly to be a strong proof of the possibility of employing our pauper population with advantage to themselves, to the benefit of proprietors, and the general improvement of our country." The remark of the *Scotsman* applies to other countries besides Scotland. Several of the lower ranges of hills in England do not ascend to the altitude of Leadhills, and in a lower latitude, the region of profitable cultivation will necessarily ascend higher, and probably in proportion *cæteris paribus* to the range of isothermal lines, or to the height of the curve of congelation in that latitude. "The Duke of Athol has ascertained, that whilst the Scotch fir thrives only at an elevation below 900 feet in the north of Scotland, the larch ascends to 1600 feet, and may ascend higher." The same fact I have often observed at Leadhills, for there Scotch firs will not grow, and all other trees are stunted excepting larches, which grow luxuriantly when protected. What a wide field for the cultivation of timber, both in England and Scotland, does no this discovery of the Duke of Athol's at once disclose? The unprofitable heaths of Scotland, where they are not cultivated, may be adorned with wood, and almost all the *hills* of England may have larches growing upon their summits. Instead of importing timber from other countries, we may then have more than we require, and thus obtain new resources from being the exporting nation.

In our next paper we shall mention the different methods that have been adopted to measure altitudes, and show how these may be simplified, so that a near-enough approximation may be easily obtained.

A A.

A POPULAR COURSE OF CHEMISTRY.

II.

ATTRACTION.

IF a stone be thrown into the air it will mount to a considerable height, proportionate always to the force employed for its projection; when this becomes exhausted, the stone returns to the earth, and remains at rest upon its surface. This simple and familiar phenomenon, is a consequence of *Attraction of Gravitation*,—a power with which the earth is endowed, and which is not only active in compelling the descent of a body to its surface, but also in holding it there at rest until it be again put into motion by some external cause. But the stone is a very hard and compact substance: its particles are held together with a very considerable degree of force, and the power which presides over its form and texture, is called the *attraction of cohesion or of aggregation*. It is exerted in different bodies with very different degrees of force, greatest in solids, as the stone,—less in liquids, as in water,—least in aëriform bodies, as in the atmosphere.

The same body is often found in various states of cohesion: thus, marble is hard, chalk is softer, whiting softest, and yet these are all precisely of the same *chemical* composition.

Different degrees of force are required to divide them. The first demands the blow of a steel hammer to break it,—the second may be broken with a wooden mallet,—and the third easily crumbles between the fingers.

The metals are all one class of bodies, but of varying degrees of hardness. Iron is hard, it scratches copper, copper will cut lead, and lead will cut potassium.

Were it not for the attraction of cohesion existing amongst the particles of bodies, form and texture would be unknown. We may illustrate attraction of cohesion, by several easy examples; two clean surfaces of lead will forcibly adhere together if pressed into contact;—if a leaden bullet thrown from the steam-gun happens to stick upon the iron target, another striking it will adhere forcibly; another and another and so on until a vast number of bullets thus cohere into one mass, which if it be cut in half and examined, appears perfectly solid, and no trace presented of its being made up of a number of spheres.

Numberless processes in the arts depend upon attraction of cohesion, and the smoother the surfaces brought into contact, the more forcible the cohesion. Two pieces of wood, if planed ever so true and smooth, will not adhere; because, from their nature they abound in minute asperities, and cannot come fairly into contact; but melted glue put between them fills up these irregularities, presenting a smooth liquid surface, and now upon pressing the pieces of wood together, they attract each other with vast force. The less glue remaining in the joint the stronger it is, on account of the closer approximation of the two solid surfaces,—a fact well known to the cabinet-maker, who in veneering, squeezes out as much *glue as possible* from between the veneer and the block upon which it is placed, so as to make the joint what is technically called "*wood and wood*." The process of making pasteboard and papier maché tea-trays, are other examples of cohesion; so also is that of candle-making, especially wax-candles. These are not generally cast in moulds, as some persons suppose, but are made by the successive applications of melted wax around the wick. The first portion poured on adheres and congeals, a second portion adheres to this, and so on, until the required size be produced. Cut a wax-candle in half, it will present a succession of cylindrical rings.

Reduction to powder, rasping or filing, are examples of overcoming attraction of cohesion. Substances which are not applicable in their original solid state, thus become of use when more minutely divided;—thus, wheat is ground into flour, so is coffee into powder, tobacco into dust or snuff. A piece of thick iron-wire held in the flame of a candle, will become red hot; but if it be reduced into fine filings, these will burn with brilliant sparks, if sifted into the flame, because their attraction of cohesion is so much destroyed that the flame has access to them in all directions, and with sufficient energy to heat them up to a point necessary for their combustion, which it could not effect when the metal was in a compact solid state.

The *chemist* is well aware of such facts, and therefore calls in the aid of the *mechanic*, who furnishes him with an endless variety of files, rasps, mortars, &c., for overcoming the attraction of cohesion of the various substances which he operates upon in his laboratory.

The grand *chemical agent* for effecting this end is *Heat*, and so universal is its application, that the chemist is not unfrequently called "The Philosopher by Fire;" and Heat is defined as the "Antagonist of Attraction." Thus, a mass of lead exposed to heat, has its attraction of cohesion overcome by that agent, it is incapable of maintaining its solid form, but melts and becomes liquid, and in this state it readily adapts itself to the form of the vessel containing it, upon withdrawing the heat, or cooling, it solidifies, and attraction of cohesion returns in full force. It must not, however, be imagined, that when the lead is liquid, attraction of cohesion is totally destroyed; for such is not the case: it is only exerted with a *less degree* of force in the liquid metal; and if this be poured from any considerable height through air, it will be cooled, and congeal into spherical drops. Thus, the small shot used by the sportsman, is formed by permitting melted lead to fall in a shower from a very considerable elevation. The spherical form is that which all liquids assume if unsupported, or supported upon surfaces for which they have no attraction. Thus, rain-drops are spherical, and these congealed present spheres of hail. We have found that solids differ in cohesive force, and the case holds good with liquids. Mercury is a fluid metal of considerable cohesive force; to pass the hand through it is not easy; but the hand will move through sea-water without difficulty, it experiences very little resistance from pure water,—still less from spirits of wine,—least of all from æther, these bodies all differing in the cohesive force with which their particles are held together.

If a cork be accurately fitted to a bottle containing syrup, that viscid *fluid will not* escape between its surface and the neck of the bottle;

but æther will readily pass between, because its particles are more attenuated and mobile. The very small degree of cohesive attraction which exists amongst the particles of aëriform bodies, must be evident by the ease with which all solids, and fluids, whatever their nature, drop through the air; and also from the very little resistance which the hand experiences in passing through the same wonderful medium.

When attraction of cohesion takes place amongst the particles of bodies, causing them to assume symmetrical arrangements, producing solids of regular and determinate figure, it is called *attraction of crystallization*; the results, *crystals*. In order to confer the crystalline state upon a substance, its particles must be endowed with freedom of motion; such end cannot be attained by any mechanical means; for, although we may reduce a body to the most subtile powder, its particles will not, even then, be able to move into mutual attraction, so as to produce a regular solid. If, however, we call in the aid of chemistry, we can easily give them freedom of motion. For example, expose a lump of the metal bismuth to heat, it melts, and extreme mobility, *i. e.* of fluidity, results. Upon allowing it to cool gradually, the particles approach each other slowly; and, just before it sets, by quickly pouring away the interior portions of yet fluid metal, the cavity of the crucible in which the experiment is made, will be found studded over with regular crystals, of a cubic form.

In this instance, crystallization consists in destroying cohesion by the solvent power of heat, and slowly permitting its return again, by cooling, or the escape of the solvent power.

But there are other solvents which may be employed, suited to the nature of the various bodies that we wish to crystallize; thus, water will dissolve common salt, nitre, or alum, and the solid particles of all these bodies will be endowed with freedom of motion throughout such solvent: this is called *solution*. Set three saucers, containing such solutions, in any warm place, and let part of the water dry away, or *evaporate;* then remove them to a cold room, and observe what happens. The particles of the substances will begin to attract each other, and form crystals; but not all of the same figure: the common salt will yield cubes; the nitre, six-sided prisms; the alum, octohedrons; and if these crystals be dissolved over and over again, they will always appear in the same forms.

The regularity of figure, however, will depend upon the rapidity or slowness with which the operation of cooling is conducted. Thus, if it be hurried, the particles are thrust confusedly into the sphere of each other's attraction; and the crystals are very small and imperfect: if it be conducted with extreme slowness, they have time to approach each other with great regularity, and form crystals of large size, and beauty of figure.

Make a solution of nitre, and evaporate it over a lamp, to about half its bulk; then set it to cool, and stir it continually; the crystals will be small and granular.

Evaporate a similar solution to three-fourths, and let it cool very slowly, which may be done by putting it on the warm hob of the grate, and letting it remain there until the fire decays, and all is quite cold, it will yield crystals very large and regular.

Epsom salts are generally found in the shops in very small crystals, because the manufacturer has cooled his solutions rapidly, in order to obtain the largest possible crop in the shortest possible time; regularity of figure is not his object; and none of the crystals weigh more than a few grains. But Epsom salts may, by slow cooling of the solution, be obtained in single crystals, weighing upwards of an ounce each, of the utmost regularity of figure; namely, a four-sided prism, with a dihedral summit. *Salt*, in the common acceptation of the word, implies culinary salt; but the chemist employs it to denote a variety of substances, totally distinct from common salt, some of which are insoluble in water, some very soluble, crystallizable, and uncrystallizable, pungent, and tasteless, fixed, volatile, metallic, earthy, and of every variety of form and colour. Thus, *marble* is a salt; so is *pearlash*, *blue vitriol*, and *plaster of Paris*. The term is not confined to that which is only *saline* to the taste. Water, added to alum, nitre, or Epsom salt, will dissolve a considerable portion of them; but at length a point will be found where it refuses to exert any more solvent power; the solution is then said to be *saturated* with the salt: if it be now heated, it will be found capable of dissolving more; heat, in this case, increases its solvent power; but another point will be found, when even the hot water refuses to dissolve any more, and thus we obtain a *hot saturated* solution; this, when cooled, deposits its excess of salt in crystals; but the *mother-water*, as it is now called, after giving birth to crystals, is a cold saturated solution.

When salts are crystallized from solutions in water, they very frequently retain a portion of that fluid, not mechanically mixed with them; but as an essential component, to which their regularity of figure and colour, in some instances, is referrible.

Thus, *Glauber's salt* contains a vast quantity of combined water, and crystallizes in six-sided prisms, transparent and beautiful: expose them to heat, the *water of crystallization* flies off, the crystal loses its shape and crumbles down into a white powder, which, if dissolved in water, will again assume its original crystalline shape, on account of regaining its water.

Crystals of gypsum, which are found in the blue clay of London, are of a glassy transparency, and of regular figure; this is due to water: heat them, they crumble into a white powder, well known as plaster of Paris, and which, when wetted with water, rapidly absorbs it, *setting* into an imperfectly crystalline solid. All plaster-casts retain their form and beauty in consequence of the water which they contain, remove it, and they fall into shapeless or *amorphous* masses.

Crystals of *blue vitriol*, caused to part with their water of crystallization, form a *white powder*, which will again yield *blue* crystals upon solution*. On the other hand some salts do not retain any water of crystallization, this is the case with nitre; nor does it attract any water from the air as some others do, hence its great utility in making gun powder. When salts attract water from the air, they are said to *deliquesce; pearlash* is an example of this. Some part with water upon exposure to air, and they are said to *effloresce*. *Glauber's salt*, for example, instead of becoming moist by absorbing water, becomes dry by throwing it off. The introduction of solid bodies, such as sticks or strings into solutions about to crystallize, induces the deposition of a larger crop of crystals by exposing an extended surface for them to form and accrete upon; they are called *nuclei;* and familiar examples of their action are found in alum-baskets, where the alum is crystallized on twigs, in verdigris, on sticks, and in sugar-candy upon strings.

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* When salts do not contain any water, they are called *anhydrous*.

Sometimes neither heat nor water can be employed for solvent agents in the manner just described; and, therefore, other means are resorted to for conferring crystalline form. Camphor is a volatile substance; and although heat will easily melt it, yet it will as easily evaporate it; and, therefore, we cannot crystallize it by heat as we did bismuth; nor is it sufficiently soluble in water. We therefore crystallize it by the process called *sublimation*, which consists in raising it very gently into vapour by heat, and thus endowing its particles with freedom of motion, and then upon cooling the vapour, they arrange themselves into crystals. This process even goes on at the common temperatures of the atmosphere, as may be observed in the show-bottles of camphor in druggists' windows; and another curious fact also becomes evident at the same time, which is, that the crystals invariably form in the greatest abundance upon that side of the bottle nearest to the light. This is not, however, peculiar to camphor, for many solutions of salts present the same phenomenon, and the manufacturer of saline bodies frequently finds that the copiousness of his crop of crystals is materially affected by the bright or gloomy state of the weather during the process. Camphor may be dissolved in spirits of wine in great abundance; if to saturation, by the aid of a moderate heat, a portion of the solution poured into a cold glass will instantly crystallize in beautiful arborescent forms.

In crystallization, therefore, when one solvent refuses to act directly, we modify its application, or employ others suited to the nature of the substance we wish to crystallize.

Crystallization is a most important process both in the chemical laboratory and in the arts, and from the circumstance of the same body always assuming the same form, we are enabled, in many instances, to pronounce upon its nature without having recourse to further examination. There are, however, some instances in which different substances have the same form; thus, *oxalic acid*, *white vitriol*, and *Epsom salts*, are all four-sided prisms; and it is very difficult to distinguish "which is which" by mere inspection; but then their chemical characters vary, oxalic acid is *sour* to the taste; white vitriol, *styptic and metallic;* Epsom salts, *bitter and saline*, and thus by merely tasting them they can effectually be distinguished from each other, independently of any refined chemical knowledge. The lamentable mistakes that the substitution of oxalic acid for Epsom salts often gives rise to, may thus be easily avoided, this caution on the part of the purchaser should never be omitted; the mere word of the druggist's assistant, in many cases, *cannot be trusted;* for very frequently his whole stock of knowledge is confined to the mere *labels* of the drawers and bottles, and he knows nothing of the *chemical* nature of their contents.

The term *crystal* was originally applied by the ancients to denote *quartz* in a regular six-sided figure; this substance they imagined to be water in a still more solid state than that of ice; it is now applied to all sorts of regular polyhedral solids*. A certain degree of attraction is observable between solids and liquids: thus, if a finger be dipped into water and withdrawn, a portion of that liquid will be found hanging from its extremity in the form of a drop. Again, if water be poured into a perfectly clean glass, its surface will not be found truly level; but cupped at the edges, where the solid glass exerts its attractive force upon the liquid and draws it from a level line. If we narrow the dimensions of the glass, the fact of this attraction becomes more and more observable; and if we greatly contract it, so as to form what we designate a tube of very small bore, we get a result yet more remarkable; for not only will the water rise in such tube, but it will be retained there, when the tube is handed about.

This effect, from taking place in narrow tubes, even in such whose calibre is as fine as a hair, is called *capillary attraction*, and it is a most important and interesting power of attraction. Take three quills (perfectly clean and free from grease), a swan, a goose, and a crow-quill, cut them all to the same length, making them tubes open at both ends, like the quill into which the tuft of a camel-hair pencil is placed, stick them on a bit of card, so as to hold them at the same level, with their orifices touching the surface of water, which will rise in the smallest quill to a greater height, and be retained in it with a greater force than in any of the others; and if even a smaller quill be used, as from the wing of a blackbird or canary, the water will be yet higher raised, and yet more forcibly retained. A cane is an assemblage of small tubes, enclosed by a hard epidermis; take a piece about an inch long, and place one end in water, the small tubes will attract it speedily towards the other end, or dip one end in spirits of wine, it will rise also, and may be burned from the other extremity, thus furnishing an illustration of the theory of the action of a wick. A bundle or fagot of small metallic wires, will also attract the spirit, and such contrivance is used for making incombustible wicks for spirit and tea-urn lamps.

Common cotton wicks may be considered as so many small cords, placed side by side, and leaving interstices which act as capillary tubes in raising liquid tallow, wax or oil, for combustion in candles or lamps.

If a skein of cotton be hung over the edge of a glass of water, it will attract it out of the glass; and if the water be muddy, the solid impurities will be left behind, and pure water only pass over. This is a most ancient, yet effectual mode of *filtration;* a process which consists in the separation of *mechanically*-suspended solids from liquids. It will not filter salt water fresh, because salt is *chemically* and not *mechanically* diffused throughout water; hence, any solution of salt filtered, is just as strong after as before the operation. Paper, when first made, is a very *porous body,* and porous bodies generally enjoy this property of attracting fluids. Thus, white blotting-paper rapidly and effectually absorbs ink, either from a pen or from writing-paper; and is accordingly extensively employed by the chemist to form filters; the simplest shape of which indispensable articles is that of a square of paper, folded into four, and afterwards opened out into a cone, so as to fit the interior surface of a funnel.

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* Quartz, is the *earth silica*, in a pure state.

In order to fit paper for writing, it is necessary to destroy this capillary attraction to some extent, and it is done by impregnating the bibulous sheets with a weak solution of size, which dries in them, and destroys their greedy force of attraction. Wood rapidly absorbs ink; and it is difficult to write a direction upon a wooden box-lid, in consequence of its capillary pores drawing away the ink from the pen in all directions; but rub it over with chalk or rosin, the pores are thus filled, and the letters indited by the pen no longer spread. Ink spilt upon a plain mahogany table, dyes deeply into its surface; but if the table be waxed, or French-polished especially, no absorption of ink takes place, because the capillary pores are thus already filled; hence, ink can be wiped completely from such surfaces, without leaving a trace behind.

Hence the utility of varnishes applied to articles of furniture The harder varieties of wood are not so porous as those which are lighter; ebony or rosewood will not absorb water like willow or deal. A lump of common salt, or of sugar, placed in a saucer full of water or of tea, soon become wet, on those parts even farthest from the main body of fluid, in virtue of the capillary attraction of their pores.

Water attracted into the fibres of a rope causes them to swell, and thus virtually to shorten the rope; a common garden line is an instance of this; when first put up it hangs in a curve; but when the laundress places wet linen upon it, it absorbs water from them, and tightens; so, if it be exposed to a shower of rain, it becomes tightly strained, often to an extent sufficient to draw the hooks out of the wall.

The threads of which linen and woollen cloths are manufactured may be considered as cords, and the shrinkage of such fabrics is proverbial, when wetted for the first time, they contract in the two directions of their intersecting threads. Twenty-seven yards of Irish linen, value two shillings per yard, shrank three quarters of a yard when first wetted with cold water.

Advantage is taken of this force of shrinkage upon some occasions. Thus, in bandaging a horse's legs, the rollers are sometimes put on dry, and afterwards wetted with lotion; the cloth instantly begins to shrink, and strong, but equable pressure, is thus applied to the limb.

Wire-gauze, if very fine, retains water amongst its interstices by capillary attraction: such is the progress of philosophy, that water can now be drawn in sieves. The labour of the personages in ancient fable, is now no longer in vain.

Capillary attraction enacts a powerful and active part in many natural operations. Much of the disintegration and decay of rocks is referrible to its action. The softer varieties attract water amongst their pores; and by the alternations of heat and cold, it enlarges or contracts, and powerfully expands in freezing, thus riving the parts asunder, and reducing the sharp angular face of a rock gradually to a rounded form.

Again, water is absorbed amongst the pores of soils, and disintegrates them by its expansions and contractions: by their absorptive power the rain or dew are conveyed from the surface to the roots of plants, in which capillary vessels also exist, which draw the liquid nutriment upwards, and transfer it amidst the body and branches of the plant, to sustain its functions of vitality

A solid body which exerts attraction for one liquid, very often exerts no attraction for another; thus the finger attracts water, a pen attracts ink; but dip either of them into quicksilver, and they fail in bringing away, or attracting, that liquid. Again, pour mercury into a glass, and it presents a *convex* surface, whereas water gives one that is *concave.*

This effect is usually designated by the term *repulsion;* the glass is said to repel the mercury; but perhaps the more philosophical expression is, that the particles of the mercury have a stronger attraction for themselves than they have for the glass, and therefore they endeavour, as far as possible, to preserve the spherical form, which thev would entirely assume if unsupported.

Water poured into a glass, *perfectly dry*, may be raised above the level of the edge, in a *convex* form; because the particles of the water have more attraction for each other than for the dry glass: wet the edge, they are then instantly attracted,—an overflow results, and the water sinks to a *concave* form. A greasy pen dipped into ink cannot be filled with that fluid, a very little only will reluctantly adhere, in minute, well-defined globules, which may easily be shaken away, as they have no attraction for the greasy surface. Cleanse away this from the pen, it will withdraw and retain ink in considerable quantity; indeed, its retention, flow, and deposit upon paper, all depends upon capillary attraction. Water thrown upon oily or dusty surfaces rolls about in well-defined globules, having no attraction for such matters. Rain-drops thus beautifully repose upon the surface of cabbage or poppy leaves, without wetting them, as they are covered with a fine pollen or powder, having no attraction for water: brush this off, and the leaf becomes wetted.

The whole art of making waterproof clothing depends upon selecting a material which water will not dissolve, adhere to, or penetrate mechanically.

Cotton fabrics impregnated with a solution of Indian rubber, have their capillary pores thus filled up, and the same substance is insoluble in water.

If a thin piece of perfectly clean silver, be laid upon the surface of mercury, it will be attracted with a very considerable degree of force, which might be very plausibly referred to the force of attraction which we have been considering; but, continue the contact for a little time, then withdraw the piece of silver, and attempt to wipe the fluid mercury away from it, as water from a stick or glass rod; it will be found impossible to effect this: hence, some power in addition to mere mechanical attraction between a solid and a fluid holds on the mercury. *Scrape the surface* with a knife; the mere mercury is not removed, but *a soft, unctuous,* metallic solid, very different from either the mercury or the silver; and if the two metals are suffered to remain in contact, they will lose their original characters, and entirely form a soft solid mass.

This attraction is particularly characterized by the change of form or state of the acting bodies; and it takes place between such as are of the most opposite natures. It is called, therefore, attraction operating upon dissimilar particles. *Chemical attraction*, or *chemical affinity*, and no mere mechanical means can enable us to separate the mercury from the silver, so as to exhibit each in its insulated state. Such separation can only be effected by chemical means, and we then find that the two metals are again presented, each enjoying the same properties as they did before their union; that they are ready to unite again at the chemist's will, again to separate, and that this union and separation may be carried on without limitation. The soft solid compound is called an *amalgam**, place it in a small glass retort, the beak of which just dips beneath the surface of water, apply the strong heat of a charcoal fire or of a spirit-lamp to the retort, the heat will overcome the chemical attraction between the metals, the mercury will *boil* and volatilize, its *vapour condenses* in the cold water, and forms globules of *liquid* metal; the silver is perfectly fixed at this degree of heat, and, therefore, remains behind in the retort pure, and free from volatile mercury.

This process is an example of what is called *distillation*, or the conversion of a body into vapour, and its condensation by a cold surface, re-forming a liquid, a process abundantly employed for extracting volatile matters from such as are fixed, as is the silver in this instance.

The formation of the amalgam by the two metals is called *synthesis*, its resolution into them again, *analysis*, or *decomposition*, which in this instance is effected by *heat*.

As all the subsequent papers which are to appear under the title of "A Popular Course of Chemistry," will be entirely devoted to the nature of chemical attraction in all its various forms and applications to science, arts, and manufactures, the subject is not further pursued upon the present occasion; the object of this paper being *attraction* generally, and mechanical attraction has been somewhat fully dwelt upon, because it is highly important that its principles should be well understood by those who are about to commence the study of chemistry.

* From the Greek, to marry together.

THE COMETARY SYSTEM.—THE COMET OF 1835.

It was the declared opinion of one Bodinus, a learned man of the 16th century, that comets were no other than the souls of illustrious men, who, having remained many ages upon the earth in the capacity of guardian angels, had been called to heaven in the shape of flaming stars. If a comet, instead of being a mere accumulation of nebulous matter, as our philosophy teaches us that it is, were, as Bodinus believed it to be, some bright intelligence interested in the destinies of our race, and commissioned, at stated seasons, to work out the designs of Providence in this nether world, how manifold are the subjects of speculation which we might assign to it, as, after each return, it again and again toiled through the years of its solitary journey.

The comet of 1835, when it came in 1456, was encountered by the anathemas of the whole Catholic Church, headed by the Pope. Dismayed at once by the progress of the Turks and the progress of the comet, Calixtus included them both in the same prayer of conjuration ordered to be said in all the churches.

It came again in 1531, and found America discovered, printing invented and in general use, and the Reformation begun*.

1607 again completed its cycle. And now the *Copernican* system had been published to the world†; the *telescope* had been discovered; Galileo and Kepler had been born, and had probably laid the foundations of their discoveries, the one in mechanics, and the other in astronomy.

Next came 1682 and the comet, and the laws of motion were ascertained and published to the world; the discoveries of Kepler were made, and Newton had built up upon them the theory of universal gravitation.

1759 was to be the next period of its appearance, and its coming was now, for the first time, *foreseen*. Halley, afterwards Savilian professor at Oxford, having undertaken to calculate the orbits of the different comets which had, up to that time, been observed, presented, in 1705, to the Royal Society, a work called *Cometographia*, in which he predicted‡ the return of the comet of 1682 in 1758, an announcement received in those days with no little surprise and interest. It was, however, immediately foreseen by astronomers, that the path of this comet would be disturbed by the attraction of the planet Jupiter. Lalande and Clairaut undertook to calculate the amount of this disturbance. The work was one of enormous labour, which they would never have undertaken, as Lalande himself admits, had not assistance been rendered to them (strange to say) by a lady. To Madame Lepaute, the wife of a celebrated watch-maker in Paris, was assigned a principal portion of their calculations, and to that lady is due a principal

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[*] This time it was accurately observed by one Apian, a professor of mathematics, at Ingolstadt.

[†] The great work of Copernicus, *De Revolutionibus*, was published in 1543.

[‡] *His words, translated,* are "Hence I dare venture to foretel that it will return *again in 1758.*"

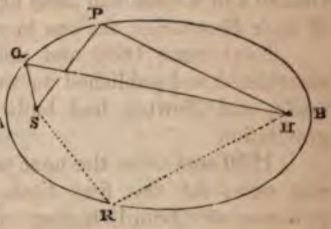
share in their success. "During six months we calculated from morning till night, even during meals," says Lalande. They determined the actual perturbations, during 150 years, of Jupiter and Saturn, and they arrived, finally, at the conclusion, that its coming would be delayed no less than 518 days by the attraction of Jupiter, and 100 more days by Saturn. The time of its perihelion passage* was thus brought to 13th April, 1759: it was, nevertheless, stated that errors might have been made amounting to a month either way.

These conclusions Clairaut published to the world in November, 1758, when astronomers had already begun to look for the comet. It was first seen by a farmer of the name of Palitzch, near Dresden, on December 25, 1758, and at Paris, on January 21, 1759. It passed its perihelion on March 13, 1759, just one month after the time predicted.

The comet of 1759 was next to complete its orbit in 1835; and of its appearance in that year an account will shortly be given, when we shall first have answered two questions, which will, no doubt, have suggested themselves to every one who has read so far of this paper. They are these:

How can any prediction of the return of a comet be made? and, supposing such a prediction to be possible, *How can it thence be shown that this comet of* 1835, *is the same with those seen in* 1682, *in* 1607, *in* 1531, *and in* 1456?

Before the first of these questions is answered, the reader must be made acquainted with the nature and properties of a certain curve, called by geometers an ellipse. If a fine thread be taken, and its two extremities fastened to two points, S and H, fixed on a flat surface, and not so far distant from one another as the thread is long; and if the thread, which will thus lie loosely between the points, be now stretched by means of a slender pencil to P, and keeping it thus stretched, if the pencil be made to move to Q, tracing as it thus moves a line PQ, that line will form part of a curve called an ellipse, the whole of which curve may be described by continuing the motion of the pencil completely round, in the direction PQR. Now the lines SP and PH, added together, are equal to the length of the string; for when the pencil was at P, the string coincided with them, and, for the same reason, the lines SQ and QH are, together, equal to the length of the string, and are, therefore, together equal to SP and PH added together; and this is the distinguishing property of the ellipse: "If, from *any* point in it, lines be drawn to S and H, the sum of these will always be the same;" thus at R, the sum of the lines RS and RH is equal to the sum of QS and QH, and PS and PH.



If now the point H be brought nearer to S than it was before, the length of the string remaining the same, and the curve be then described as before, it will be found to have altered its form, so as more nearly to approach that of a circle, as shown in the second figure, and H may be brought so near to S, that its form shall scarcely be distinguishable from that of a circle, which figure it would indeed manifestly become, if the point H were made accurately to coincide with S.

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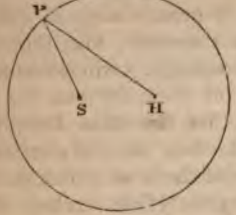
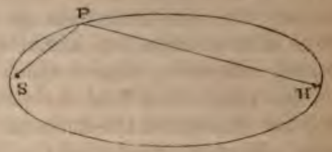
* This term will be explained in the course of this paper.





Again, if instead of being moved nearer to S the point H be moved further from it, then the ellipse will be found to have assumed a form like that of the third figure, not nearer to, but further from, the form of a circle than it was at first. Ellipses whose foci S and H are near one another, and which, therefore, approximate to circles, are called ellipses of small eccentricity. Ellipses whose foci are further from one another, and which, therefore, deviate more from circles, are called ellipses of greater eccentricity. These curves have been supposed to be described with a piece of thread short enough to be conveniently used for the purpose. But curves may be imagined to be described according to the same law, and having, therefore, the same *properties*, traversing vast and inaccessible regions of space, and in which dimensions, which we have taken to be inches, are replaced by millions of miles.

The properties of the ellipse have, even from a very remote period*, been the subject of careful study among geometers, and their acquaintance with them is so far perfected, that knowing certain circumstances with regard to *any portion* of an ellipse, or having certain *data* (as it is termed) in respect to that portion of an ellipse, they can tell the form and magnitude of the whole of the ellipse. Having these data of any, the least part, they know certainly what is the whole of the ellipse of which it forms a part.

Now, *four* observations upon one of the heavenly bodies, describing an ellipse†, are sufficient to give an observer at the earth's surface, these data. Thus then four observations tell him what is the ellipse, which, if it describe an ellipse, a comet is describing. Now, knowing the *form* and *magnitude* of the ellipse, he can further, by another known process of calculation, tell all the circumstances of the comet's motion in it; and if it really move in an ellipse, he can, therefore, tell beforehand, what place it will occupy in it, after any given time.

Suppose him to have done this, and to wait until that time, and again then to observe it. If his observations agree with the prediction, he will know that he was right in supposing the comet to describe an ellipse—and *that particular ellipse.* Now, observations of this kind have for the last two centuries been made upon all the comets which have appeared, one hundred and thirty in number, and the observations on each have been repeated so as to verify one another in a great variety of different ways; and the conclusion from all has been the same; viz., that those portions of their orbits, which the comets are describing when within our sight, are ellipses*; ellipses which have all of them the sun for their focus, or rather, for one of their foci,—and that the other focus is infinitely far off, beyond the limits of the orbit of the most distant of the planets. Moreover, that all these ellipses are of the kind which we have described as of *great eccentricity*, or deviating greatly from circles. Now, similar observations applied to the planets of our system, show them also to describe ellipses, having, too, the sun in one of the foci of each ellipse; but these ellipses are of exceedingly *small eccentricities*, or they approximate very nearly to circles.

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* Apollonius Pergæus, the author of a most learned treatise on the curves, called Conic Sections. of which number is the ellipse, flourished in the second century before Christ.

† A comet can only be seen by us when it is describing that portion of its elongated orbit which is nearest the sun; now this portion of its orbit coincides very *nearly with the* corresponding portion of a certain other curve, called a parabola, and *three observations* are sufficient on the supposition that it is a parabola.

But the elliptic orbit of a comet may lie in an infinite variety of positions in respect to the sun, and yet in all these have its focus in the sun. The length of the ellipse may lie one way or another, to the right or the left of a line drawn, for instance, from the sun to a particular star, or at any angular distance from that line, or having its plane inclined, at one angle or another, to the plane of the orbit which our earth describes round the sun; and all these things we are required to know, before we can fix what is the precise path in space along which the comet goes. They are called the elements of its orbit. And on the other hand, knowing these, we do know precisely the curved line which through the years, perhaps centuries, of each of its revolution, the comet is describing through the fields of space. Nay more, we can tell precisely what part of that path it is at any given time describing; the inward eye remains, as it were, fixed upon it, long after it is beyond the reach of the most powerful telescopes. We can tell when it will slowly reach its greatest distance from the sun, or its *aphelion*, as it is called. Somewhere, perhaps, double or treble the distance of Uranus from us; and we can tell precisely when it will go through its perihelion, or that extremity of its orbit in which it is nearest to the sun and to us. Now these other elements of a comet's orbit may all be determined from the same four observations which ascertained its form and its magnitude.

These things have been calculated in respect to one hundred and thirty-three comets, which have appeared at different periods of the two last centuries, and of one hundred and thirty of these no two are found to describe the *same orbit*,—no two of them are, then, different returns of the same comet. But if two comets, appearing at different periods, had on examination been found to be describing, one of them at one period the same path in space, which the other did at the other period; if, moreover, the actual motion of the first comet, known from a previous knowledge of its orbit, ought to bring it precisely to that point of its orbit, where the second comet was, at the *time* or near about the time when it was seen there, then we should have known that the two comets were, in fact, *one and the same comet.*

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* *This observation* will in the course of this paper be qualified.

Now, although out of one hundred and thirty, no two have thus been found to be the same; yet in the whole number, one hundred and thirty-three observed, there were three, the identity of which with three others was established. Of these, one is the comet of 1835, or 1759, called Halley's comet, because he first established its identity with the comet of 1682, 1607, and 1531; another is the comet called, for a similar reason, the comet of Encke, and the third is the comet of Biela; the first has a period of about seventy-six years, the second of three years and three-tenths, and the third of seven years and three quarters.

Thus, then, we know that there are at least one hundred and thirty *different* comets revolving continually about the sun, that number of different comets *having been seen* during the last two hundred years. None of these, except three, have as yet had time to return to us; these three *have* returned severally at their appointed periods.

How many other comets there may be, or what is the whole number of bodies which compose the *cometary*, as distinguished from the *planetary*, system of our sun, we know not. Comets have been *observed* by astronomers only during the two last centuries; one hundred and thirty different ones have during that time been seen, and more are continually discovered, as instruments are perfected and observations multiplied*. Nevertheless, hundreds may, during this period, have escaped observation. Because of their distance, the faintness of their light, or because we cannot observe the heavens in the day, they traverse them so rapidly, that long before the period of the year when that portion of the sky in which they move becomes visible, they are gone†. The comet of Biela could only be found by Sir John Herschel, "with a reflecting telescope of twenty feet in length, an instrument of enormous power in the collection of light." What shall we say then of the number and variety of the cometary bodies, which might have been discovered, had we instruments of greater power, were our observations more numerous, and carried back through a greater distance; or what shall we say of the possible number of cometary bodies which may be discovered during the two next centuries? It is quite within the bounds of possibility, that the number of the different comets, revolving continually round the sun, may amount to thousands.

Those which are known to us have their orbits lying in every conceivable position in space, subject all, however, to the condition, that one of their foci is occupied by the sun; they have their planes inclined to one another, and to the plane of the earth's orbit, at every possible angle up to ninety degrees, and the lengths of their orbits are directed towards any and every point in space; moreover, and this is a singular fact, they have the directions of their motion some one way and some another. Thus, one comet revolves in its orbit eastward, and another westward. Moreover, by reason of the elongated forms of their orbits, and their various directions in space, these orbits are made continually to cross one another, and the orbits of the planets, and comets are thus frequently brought into such positions, in respect to the planets, that the attraction of these greatly interferes with, and controls, the attraction of the sun upon them.

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* Scarcely a year passes in which one or more new comets are not discovered.

† It is related by Seneca, that during a great solar eclipse, sixty years B. C., a *large comet* was seen near the sun.

Now, in all these points of view, the cometary is distinguished from the planetary system of the universe. The orbits of the planets are all of exceedingly small eccentricity, they differ little in form from one another, and none of them much from circles. Their planes are none of them inclined much to one another, or to the plane of the earth's orbit*. Their orbits never intersect one another, and their distances are such, that the attraction of the rest upon any one must always be greatly less than the attraction of the sun upon it; moreover, all of them describe their orbits *the same way*, or in the same direction, towards east.

These differences between the system of the planets and the system of the comets, are not without a reason; they involve another and infinitely important distinction between the two systems. The system of planets is *stable*, the system of comets is *unstable*.

These are terms which must be explained. All the bodies of our system (and from recent observation it appears of every other), attract one another, each planet is attracted by every other planet, as well as by the sun, and in reality moves more or less in consequence of, and in obedience to, each such attraction, deflecting more or less, continually, from the path which it would otherwise describe, according to the greater or less proximity of the disturbing body. And the aggregate result of these disturbing motions is, an orbit whose general character is that of an ellipse, but which is not in reality an ellipse; an orbit, which moreover is continually changing, no two successive orbits of a planet round the sun, being exactly the same. This continual alteration in the paths of the planets through space, might go on with more or less of rapidity, and it might be such, as in its nature, would go on *infinitely*, so that we might be assured, that our system should never again be what it now is. Nay, a state of things may be imagined, such as would produce a continual change of this kind, leading necessarily and ultimately to its entire destruction. Now we are assured by the most certain reasoning, that the state of things which actually exists, is *other* than this—that it is a state of things, which renders it *impossible* that the forms of the planetary orbits should continue to change for ever; that on the contrary, the existing state of things renders it absolutely certain and necessary, that (if nothing else interfere) eventually, after perhaps millions of years, each planet shall again be describing the very same path that it is now describing, and the whole order of planetary disturbances return from period to period, by almost imperceptible degrees, and in an eternal cycle. This condition of the system, is that which is meant by its stability, as the opposite condition is implied by instability.

Now the peculiar circumstances out of which the stability of the planetary system arises, are precisely those in which we have described it, as distinguished from the cometary system. They are the great excess of the attraction of the sun upon any of the bodies which compose it, as compared with that of any other body. The uniform direction of the revolutions of the planets in their respective orbits, towards the east, the small eccentricities of their orbits, and their small inclinations to one another. From these *provisions* in our system, it arises, that it is stable, *and if any one of them* were wanting, it would be unstable. Now in the cometary system, not one of these obtains; it is therefore unstable, and in a state of continual and rapid change, and thence arises the great difficulty of calculating the motions of the comets.

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* The *inclination of the orbit* of Mercury to that of the Earth is greater than that *of any other of the seven greater* planets, and it does not much exceed seven degrees.

The masses of such comets as have been observed are all exceedingly small, indeed it would seem infinitely small*, as compared with those of the planets of our system; so that although they exercise no perceptible influence on the motions of the planets, however near they approach them, yet do the planets exercise a very sensible control over theirs. A comet was discovered in 1770, and its orbit was calculated by Lexel, to be described in 5½ years. At the expiration of that period, it was however looked for in vain, and it was called Lexel's *lost comet*. Years afterwards, it was shown by Laplace, that this comet, when returning to keep its appointment with Lexel, had passed so near to Jupiter, that the attraction of that planet upon it, had become 200 times as great as the attraction of the sun, and the result was, that the form of its orbit had been so completely altered, that from 5½ years, it came to be an orbit described in 30 years. The attraction of Jupiter upon this erratic comet, actually brought it between that planet and his satellites, and yet, so small was its mass†, so wonderful its tenuity, that it produced not the slightest alteration in the motions of any one of them.

Every comet when it enters our system has its orbit more or less changed by the influence of the planets, and in some cases, that influence is felt throughout the whole of the comet's course. Thus, the comet of 1835, never, throughout the whole of its course, extending three billions of miles from the sun, escaped the sensible attraction of Jupiter.

Thus, then, it appears, that those changes which, in respect to the orbits of the planets, are necessarily, and must ever be *gradual*, and almost imperceptible, are, as it regards the orbits of the comets, not only perceptible, but remarkable, and moreover that, whereas the changes of the planetary orbits must return in certain cyclical periods for ever, those of the cometary orbits will not; so that what the cometary system is at any given time, it can never (that is it cannot, except by an infinite improbability) be again; but, to what this perpetual series of change tends, or in what it will terminate, no one has, probably, been bold enough to make the subject of his speculations.

Enough has been said, to show that the calculation of the motions of a comet is no easy matter. The attractions of five bodies, all of which, except one, are continually moving, upon another, which is itself also perpetually in motion,—these attractions, each of them varying, with each change of distance, their effects in accelerating or retarding the attracted body, or in altering the path which it has described,—effects to be considered and allowed for, during a period, not of some few weeks or months, but through seventy-six long years; this is a task, about which are accumulated difficulties of no common order. It is a work of infinite complication, learning, ingenuity, and labour; nevertheless it was undertaken and accomplished in respect of the comet of 1835.

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* If Halley's comet had been the 20,000th part of the mass of Jupiter, Laplace has calculated, that it would have produced an effect on the motions of that planet, which would have been in 1682, distinctly perceptible with our instruments, and in 1835, it would have been perceptible even had the mass of the comet been much less. If the comet of 1770 had been the 5,000th part of the mass of the Earth, it would have perceptibly lengthened our year.

† A distinction must be made by the reader between mass and dimensions: *mass has reference* only to the quantity of matter; and thus a body may have a very *small mass, and yet* very great dimensions: this is the case with the comets.

The comet of 1835 was, in its last revolution, influenced appreciably by the attractions of the four planets, Jupiter, Saturn, Uranus, and the Earth, and of course by the attraction of the Sun; and MM. Damoiseau and Pontécoulant severally and independently undertook the task of calculating their amount, and separately completed it. M. Pontécoulant found that the action of Jupiter would, as compared with the last revolution of the comet, on the whole accelerate it 135.34 days; that of Saturn, retard it 51.53 days; that of Uranus, retard it 6.07 days; and that of the Earth 11.7 days. The principal portion of the influence of the Earth on its motions, dating as far back as the year 1759, or the very beginning of its revolution, at which time it passed very near the Earth.

Allowance being made for these, the whole period of the comet's last revolution was brought to 27937 days, and counting from the 13th of March, 1759, when it last passed through its perihelion, or nearest extremity of its orbit to the sun, this brought its next perihelion passage to the 13th of November, 1835*. At the same time, M. Pontécoulant expressly stated, that there might be an error of a few days in this time, and assigned as a proximate cause of such an error, a possible incorrectness in the assumed masses of some of the planets. His words are, "We must here once more repeat, that it is not pretended that the time announced for the comet's return to its perihilion may not be in error some days." Elsewhere he says, "Thus then it is *conclusive*, that *about the middle of November*, 1835, the passage of the comet through its perihelion will take place."

The determination of the time when the comet would first *appear*, was altogether another and a much less important matter.—It depended upon the time when it would enter that portion of the heavens then visible at night, at that particular place—it depended upon the intensity of its light, as compared with what twilight there might be when it first appeared—it depended upon the state of the atmosphere. All these were variable elements, and none of them, except one, could be calculated upon with any certainty. It would have been madness to have mentioned a *particular day* when it should first be seen—nevertheless, both M. Pontécoulant and M. Damoiseau, ventured to announce it as probable, that it would appear during the first days of the month of August.

The facts which astronomy reveals are so stupendous, her results so far beyond the range of ordinary thought, the steps which she takes through time and space so rapid, leaving even imagination far behind, that of all sciences, she would find least credit with the world, were it not for the predictions to which she appeals, and which everybody may verify. It is this prophetic power which constitutes her strength, her whole strength, or her weakness with the vulgar. Driven from every other test, there were people disposed to cavil at this science, (as there are always people disposed to cavil at what they do not understand,) who had fixed their criterion in the predicted return of the comet of 1759.

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* M. *Damoiseau fixed its* perihelion passage to the 4th of November.

Now what was the result? It had been announced that the comet would probably be visible during the first days of August. *It was seen on the 5th of August, at Rome*[*], *by MM. Dumouchel and Vico*, its light being then exceedingly feeble. But more than this, the *precise place* in the heavens which the comet would occupy on every day whilst it should be visible, had been calculated and announced beforehand, *and it was when they directed their telescope to that point in the heavens which had been so predicted for the 5th of August, that MM. Dumouchel and Vico saw it.* It had been foretold that it would pass its perihelion on the 13th of November, that there might be an error of a few days, but that, nevertheless, it certainly would pass it about the middle of November. *It passed its perihelion on the 16th of November.*

It had been assigned by M. Pontécoulant, as a reason for the uncertainty which he thus felt in respect to the time of the perihelion passage, amounting, however, only to a few days, that the masses usually assigned to some of the planets by astronomers, and used by him in his calculations, might require correction. Of all the planets, Jupiter exercised the greatest influence over the motions of this comet. Any error in the mass which had been assigned to Jupiter, would, therefore, most affect the result. Now the mass he had assigned to Jupiter, was such, that 1054 such masses would equal the mass of the sun. Recent observations have shown, that the mass of Jupiter repeated only 1049 times, would equal the mass of the sun; and it has been ascertained, that *if M. Pontécoulant had used in his calculation this corrected measurement of the mass of Jupiter, instead of that which he did use, it would have protracted the predicted time of the perihelion passage three days, and brought it to the 16th, and to within six hours of the time when it actually took place,—an error of six hours in a period of seventy-six years!*

In our next Number, we shall give an account of some very remarkable appearances in the Nucleus of this Comet.

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[*] The reader need not be reminded how pure and clear is the atmosphere of Rome.

ON THE
ELEMENTARY STUDY OF GEOMETRY AND ALGEBRA.
II.

We have offered a few desultory observations on the very various, if not actually inconsistent, opinions which have been broached by eminent mathematicians with respect to a comparative estimate of the algebraic and geometrical methods, and the distinction between their essential principles. Our thoughts have been especially directed to the subject from the recent appearance of two small productions, both bearing, though in different ways, on these topics, so interesting to all who duly appreciate the importance of these first elements which form the basis of all accurate science: we allude to "Thoughts on the Study of Mathematics," &c., by the Rev. W. Whewell, of Cambridge; and a tract printed by the Ashmolean Society of Oxford, entitled "On the Theory of Ratio and Proportion," by the Rev. B. Powell, Savilian Professor of Geometry. The former publication embraces a much wider range of subject than the latter. But in reference to the specific question now before us, we must observe, that the professed object of Mr. Whewell's argument throughout is rather of a practical character; being directed to the question of the best topics and the best methods of elementary instruction. Whereas in the latter the inquiry is wholly remote from such questions, and refers solely to mere abstract principles. If, then, in the discussion of such practical questions, some views of the nature of proportionals, and of Euclid's system, are introduced in the one, which may be apparently contradicted by what is urged in the other, it should be borne in mind how totally different are the grounds on which the authors respectively take their stand, and the objects to which their arguments are respectively directed.

Mr. Whewell throughout contends for the importance of clear conceptions of the *things* which are the subjects of reasoning, over and above the *definitions*, in all parts of pure as well as mixed mathematics. Among other instances, he exemplifies it in the case of proportion; and all along refers to the *numerical* as the only clear or definite view of proportionals. "It is clear," he says (p. 13), "that we are capable of readily forming a distinct conception of proportion, and that a very slight suggestion is sufficient to call up this conception in our minds."

Proceeding, then, to the usual numerical definition, "we find that when we have done this, we cannot abstain from applying the same conception of proportionality to cases in which one quantity is neither multiple, part, nor parts, of another; for we readily allow that the diagonals of two squares are proportional to their sides. Thus, the conception is immediately extended beyond the limits of the definition; an undeniable proof that the conception is not the creature of the definition."

On this we will venture to ask whether if we "allow" these truths, and "extend" the conception beyond the definition, we are not doing both *wrongly?* Whether we are not taking for granted the proposition *without proof; and* transgressing all rules of geometrical reasoning in implying or imagining anything not rigidly warranted by the terms of our definition? We will not press this distinction too closely, because we are aware the author is referring rather to the best practical mode of illustration to a learner.

However, the conception being thus formed, certain self-evident properties are seen to belong to it. Some of these are stated, which are essentially numerical, and therefore follow rigidly from the numerical definition, without regard to any other conception; but he says the fifth definition of Euclid, and the demonstrations founded upon it, are usually considered by learners as obscure and confused. He conceives "that this impression arises, in part, at least, from the attempt, which in this case was made by Euclid himself, to reduce the subject to definition alone."

The author afterwards goes on to observe:—

"If proportion had been separately defined, so as to bring the conception of it before the mind, I conceive that the assertion of this fifth definition would be assented to without difficulty, as an axiom. But then, what definition of proportion shall we take?"

In order to remove the difficulty attending the choice of a definition, the author considers the introduction of a few examples will suffice, or a reference to the definitions of ratio and analogy.

"If we take any other course, we either run into the apparent confusion and complexity which, as has been stated, arises from mixing, in the fifth definition, the character of definition and of axiom, or, on the other, taking as our definition, the one first mentioned—and running from it to cases to which it does not apply, we transform our mathematics from a praxis of logic, into an example of the most loose and inconsequent reasoning possible."

Mr. Powell puts his general statement of the question thus:—

"The doctrine of ratio and proportion, then, is introduced by Euclid as a part of his system of geometry; and the student seldom fails to remark that, in the treatises on Algebra, the same subject is presented under a considerably different form; though he is usually quite unable to determine wherein the essential difference consists; and would probably find but few teachers, who could precisely point out the distinction to him. Some of the best modern writers on geometry (as Legendre), omit it altogether in their elementary systems; and most teachers in this country, though, in general, they follow Euclid, yet pass over the 5th book; and adopting the doctrine of proportionals, from algebra, proceed to apply it to the theorems of the 6th book. Euclid, however, for some reason, thought it necessary to proceed otherwise. He establishes, by a totally different method, *some* of the same properties of proportionals as the algebraic writers do; but where he stops short (not even proceeding to certain of the simplest and most universally important properties), they, on the other hand, continue the subject, or make those properties fundamental. Of Euclid's design and principles of investigation, various opinions have been held among the moderns; and some of those who most profess to be his admirers and followers, have made attempts, as they conceive, to improve upon his method; and have devised various plans for treating this portion of the subject, in order to avoid what they consider the unnecessary abstruseness and prolixity of the 5th book.

"*It might*, perhaps, in the first instance, be imagined that the method *adopted by Euclid* in his 5th book, was the first and imperfect attempt of science, as yet in its infancy, to give a general investigation of the doctrine of proportion, which, however prolix and cumbrous, ought yet, on that ground, to claim our respect and admiration; but that, the advance of modern science having furnished us with the more comprehensive method of Algebra, we may properly discard the older and more difficult process, as doubtless Euclid would have done, had the better method been known to him.

"But what is the fact? We have only to look into his 7th book, and we there find the whole doctrine, as applied to the case of *numbers*, demonstrated quite independently of anything in the 5th book, on principles, though presented in a different form, yet in substance almost identically the same as those used in modern algebraical treatises: and it would have required no more than a very obvious extension of the method, so far to generalize it, as to make it applicable to the geometrical figures considered in the 6th book, in the same way as the modern writers have done."

Now in many relations established in geometry, as subsisting between straight lines forming parts of certain geometrical figures, as, for example, the side of a square and its diagonal, we find, that if we conceive the lengths of the lines measured by any numerical scale, and express one line by an exact whole number, we shall only be able to express the other by a *square root*, or some number which has a *fractional remainder;* however small this remainder be, yet it makes the relation not mathematically exact, and two quantities so circumstanced, are called "incommensurables." Of all this, however, nothing appears in Euclid's mode of treating the subject, and it arises wholly out of the adoption of the arithmetical measures.

Thus, various relations being established in geometry between lines constituted under given conditions, as parts of geometrical figures, if we choose to adopt the idea of expressing these lines by *numerical measures*, we are then brought to the distinction of such lines being in some cases commensurable in their numerical values, in others not so. Their *geometrical relations*, however, are absolutely general, and do not refer to any such distinction.

Euclid, in his earlier books, when treating of such properties as those alluded to, does not introduce, either explicitly or by implication, the term or the idea of incommensurability: neither is it introduced in the 5th or 6th books. After having in the 7th treated of the properties of arithmetical proportionals, where the terms are all supposed expressed in exact finite numbers, and having in the 8th and 9th books continued this subject to various properties of numbers, he comes in the 10th book, for the first time, to introduce the notion of a distinction between commensurables and incommensurables, in geometrical magnitudes expressed by numerical measures. In the 11th and 12th books again, he drops al reference to this distinction, recurring to the principles of the 5th book: but resumes the consideration of it in the 13th, and applies it to various properties.

In Euclid's mode of treating proportionals, we find the same total and systematic exclusion of all reference to numerical measures. Yet many mathematicians have contended that the idea of number enters essentially into our *conception* of ratio and proportion, and must be *supposed, in order to a right* understanding of Euclid's method; which they contend was adopted for the express purpose of including the case both of commensurables and incommensurables. The quantities considered by Euclid in this book, it is true, may be of either kind; but he does not specifically regard them with any reference to this distinction. On the contrary, without pointing at all to any such consideration, it seems to be his object to regard *quantity* in a far more general point of view. "In a word," says the Savilian professor, "I conceive it clearly evident, that Euclid's specific object in the 5th book was, *not to include incommensurables, but to* EXCLUDE *all reference to the idea of* NUMBER."

But how is this done? The author's opinion is, that the nature of the quantities here discussed must be understood according to the most general and abstract notion implied by the term: in his own words;—

"Now our idea of QUANTITY IN GENERAL is very easily understood if we simply consider it in the same light as other general ideas, and only bear in mind and apply to this case the same process of abstraction by which we arrive at those ideas. We form ideas of so much in length, so much in area, so much in solidity, so much in duration, so much in number, so much in velocity, &c. From all these particular ideas we abstract what belongs to the particular nature of length, of surface, of capacity, of duration, of number, &c., and thus form our abstract idea of 'so much,' or of quantity in general, a comprehensive generic term, including under it all the particular species of quantity.

"In the first part of geometry, (i.e. in the first four books of Euclid,) we consider *quantity of extension*, and its properties in regard to linear and superficial space as referring to certain geometrical figures. And it is essential to observe, that the estimate we form of the magnitudes, consists in simply considering them as *so much length* or *so much surface*, without assuming any particular scale or standard of measurement: we compare such lines and surfaces together, and establish relations of equality between them as constituted under certain given conditions. And these form certain relations which are strictly and properly *geometrical* as not being determined with reference to any other assumed standard of measurement.

"We proceed in like manner, in the 5th book, to investigate certain more varied and comprehensive relations which subsist between quantities considered all along in the most general and abstract point of view, and which give the properties of proportionals. Nothing is introduced which refers to any of those particular characteristics by which the different species of quantity are distinguished: the conclusions, therefore, are absolutely general, and may subsequently be applied indifferently to any of the particular species. This is accordingly done in the 6th book, with regard to linear and superficial extension. And in the 11th and 12th to solid extension. The very essence of the subject treated in the 7th and other books, requires a different and more restricted mode of investigation, as applying to properties peculiar to one species of quantity: viz, number: and to certain highly curious relations which subsist when number is employed to measure extension."

But the discussion of these properties, which arise wholly out of the numerical system of measurement adopted, is studiously kept entirely distinct from the purely geometrical investigations. Those of Euclid's books, in which the numerical principle is introduced, may be taken as an entirely distinct arithmetical treatise: and there can be little doubt they originally did actually form a separate work. There is never the least intermixture of the methods, nor is anything in the geometrical books in the smallest degree dependent upon these others. Here all such ideas are totally excluded.

In elementary algebra mere addition and subtraction is not limited to any particular species of quantity; nor does it of necessity suppose the quantities concerned to be numbers. But, in the common systems, as soon as we come to introduce the idea of multiplication, there, by the very definition, we of necessity limit our consideration to quantities, one at least of which is a *number*, and usually both. This refers to algebraical proportionals and to all their applications, that is, to the whole superstructure of modern mathematical science. The doctrine of numerical proportionals, as established in common algebra, comprises corresponding particular cases of all the properties delivered by Euclid of quantities in general. The investigation, of course, gains in brevity what it wants in generality. But it extends to a greater number of properties: and is principally distinguished in that it embraces the equality of the products of the extremes and means of a proportion. This, with others dependent on it, since they involve the idea of multiplication, of course have no place in Euclid's system. The analogous property of rectangles (established in the sixth book) does not supply the deficiency, because it applies only to one species of quantity, viz.—superficial area, and not to anything equivalent or analogous to a *product* between abstract quantities.

The whole subject has a direct bearing on the question to which we at first referred, as to the essential distinction between the nature of geometrical and algebraical investigation. And the question is well illustrated by a passage in the mathematical correspondence of Dr. R. Simson (given in his life by Dr. Traill); his friend and pupil Mr. Scott had remarked, in a letter to him,

> "The fifth book is not peculiar to geometry, but is equally applicable to quantity as it is to magnitude: nor does the doctrine of proportion receive any evidence from its being expounded by lines. The symbols of algebra would, I think, be of use here, &c."—(p. 115.)

To this Dr. Simson replies,

> "As to the 5th book, I do not see that the demonstrations in it can receive any help from algebra: and the straight lines made use of in it, make the demonstrations clearer and easier than they would be without them. I do not understand your meaning, when you say any analogy may be called algebraical, as well as geometrical. The expressing lines by a single letter does not make analogy or anything else algebraical, any more than when they are expressed by the two letters at their ends, nor do I think any thing can be called algebraical, when no operation peculiar to algebra is made use of. I should be glad to see an algebraical demonstration of some proposition in the 5th book, eq. the 17th, &c."—(p. 121.)

The author, in quoting this passage, observes,

> "The question, *What* operations *are* peculiar to algebra in reference to *proportionals?* still remains to be answered. 'Such as depend on the *numerical* measure of ratio' is my reply; and I am convinced would have been Dr. *Simson's.*"—*(note* p. 60.)

He remarks also,

"Dr. Trail (in continuation of a passage before quoted) appears to me to come very near upon the truth, without actually explaining it, when he says;—'an analysis or demonstration in which many combinations of ratio are employed, may generally, indeed, be much shortened by admitting *multiplication*, division, or other operations *peculiar* to arithmetic or algebra; but the investigation or demonstration *then becomes truly algebraical*."

Upon the whole we must observe, that to do justice to this subject, considered in an abstract point of view, it would be necessary to enter much more deeply into it than would be consistent with our limits. We here content ourselves with a mere attempt to lay before our readers the views of the writers we have referred to.

In a practical point of view, the question is a very important one to all who are interested, whether in preserving the exactness and logical utility of mathematics, as a study for the improvement of the reasoning powers, or in simplifying its acquisition as preparatory to its practical applications in all branches of science. For the latter purpose it is obvious to us, that by far the easiest method is for the student to acquire merely a few of the most fundamental geometrical truths from the first book of Euclid: then apply algebra (assuming the approximate process for incommensurables), and so dispense, in fact, with all the remaining geometrical demonstrations, and go at once to trigonometry and co-ordinates, which will be an ample preparation for all elementary applications in mechanics, astronomy, &c. To the leisurely academical student of course a different and more strictly logical process is highly important, if not alsolutely essential, for deriving any of the benefits of a praxis of reasoning.

ON THE CULTIVATION OF THE STRAWBERRY.

Choose an open piece of ground, in a sunny situation. To prepare it for the reception of the strawberry-plants, open a trench full three feet deep; place in the bottom a good coating of rotten but unexhausted manure; trench the ground, continuing the same process till you have a patch prepared sufficiently large for your purpose. Before you plant your strawberries, dig in another coating of manure, and just bury it well under the surface: plant your strawberry-plants at eighteen inches or two feet distance; this must depend upon the sorts that you are planting. If the ground is a good, holding, loamy soil, the strawberries will thrive the best; but in any tolerable soil they will thrive with this treatment. The roots of the strawberry dive deep into the earth, and just at the time the fruit is ripening, will receive support from the lower coating of manure, and swell off to twice or three times the size that they usually attain. This process has been pursued by the writer with eminent success; and his friends who have tried it, have been delighted with the plan. Keen's seedling, the roseberry, and some of the scarlets, are the best strawberries for a general crop, but the old pine has never been equalled for flavour.

DESCRIPTION OF

AN APPARATUS FOR INDICATING THE INTERNAL TEMPERATURE OF ANIMALS AND VEGETABLES; AND OF THE MODE OF USING IT.

FROM THE FRENCH OF MM. BECQUEREL AND BRESCHET [*].

The whole of the experiments which have been made, up to the present time, upon animal and vegetable temperature, have been few in number, and remarkably inconclusive. The means employed have not permitted the comparison of a large number of observations; in fact, the thermometer, the only instrument which could be used, has hitherto been merely introduced directly into some parts of the animal structure. Was it desired to penetrate into the interior of an organ, incision became necessary, the state of the organ was consequently affected, and the irritation produced calorific effects which it was impossible to distinguish from those which were due to vitality. Again, the thermometer, however small the bulb, has the inconvenience of presenting a mass always sufficiently great to absorb a very large quantity of heat, in order that its temperature may be equal to that of the adjacent parts. If these parts do not instantaneously recover the heat they have lost, there necessarily results a depression of temperature. The thermometer also cannot indicate sudden changes of temperature, as it requires several minutes to become of the same temperature with the circumambient media. If, for example, a thermometer is placed in the mouth, three or four minutes must elapse before it acquires the correct temperature. Now, if during this time any thermo-physiological phenomena of transient duration should occur, it would be impossible to observe them.

It is further to be remarked, that although it is possible to introduce the thermometer, by means of incision, into certain regions, it is not so into the organs essential to animal life, such as the heart, the lungs, the liver, the brain, &c. It is, however, here that the physiologist is the most interested in knowing how the temperature is modified by motion, by the developement of the passions, the application of certain agents, &c.

Still further is it important to physiology and the curative art, to solve all questions relative to animal heat; to determine, for instance, the difference which exists between the temperature of an organ in its healthy state, and that of the same organ when altered by disease, and to ascertain the means which must be employed to remove the difference.

To examine animal heat, for the purpose of arriving at the ends pointed out, nothing more is necessary than to introduce a delicate needle or metallic probe, similar to those used in acupuncturation [†]; there is, in fact, no other means of traversing with impunity the greater part of the organs of animals. It is easy to construct this probe so as to obtain thermo-electrical effects which give instantaneously, and with great exactness, the temperature of the medium in which the probe may be placed; it is only necessary that it be composed of two others of metal, soldered together by one end of each, and the remaining ends placed in communication with the extremities of the wire of an excellent thermo-electric multiplier. The smallest possible change of temperature at the point of junction produces an electric current, which, acting upon the magnetic-needle, makes it deviate a certain number of degrees. The angle of deviation shows the temperature of the probe, and consequently that of its ambient medium.

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[*] It is conceived, that this useful application of some of the recently discovered properties of electricity and magnetism, is one of the most happy of the present day. Its ingenuity and importance can scarcely be exceeded.

[†] *Bleeding by making* punctures with needles.

Researches of this nature require, that an anatomist who can skilfully introduce the probes into the animal organization, should co-operate with the "*physicien*[*]."

An apparatus intended to measure the temperatures of this nature should be so constructed, that the part which is inserted should not communicate, or at least should communicate with difficulty the heat which it may acquire to the rest of the apparatus; if this condition be not observed, there is always ground for fearing that a lower temperature than the true one may be taken.

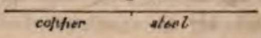
With the common thermometer, effects of this kind need not be apprehended, for as the glass is a bad conductor, the loss of heat from the tube is very small.

When metallic pyrometers are used, the results are not always certain. For example, if we suppose a metallic bar, one of the ends of which is exposed to a focus of heat, and the other, in connexion with a pyrometer influenced by the dilatation of metals; we find from the instant that the exposed end begins to be heated, it communicates to the parts adjoining, and, finally, to the atmosphere, a portion of the heat it has obtained; if the dimensions of the bar are such, that the quantity thus dissipated cannot be instantly replaced by the focus, the result is that the Pyrometer gives an incorrect indication only, and it is evident that the dimensions of the bar will be proportional to the quantities of heat imparted in each instant. Similar considerations must be attended to in the metallic probes intended to ascertain animal temperature. By giving them the smallest possible diameter this source of error may be avoided.

An excellent thermo-electric multiplier, and some probes formed of two different metals soldered together, are indispensable instruments in these inquiries. The multiplier should be so sensible, that in connecting the two ends of the wire which forms its circuit, with an iron wire soldered end to end, a difference of one-fifth of a degree of Fahrenheit between the two solderings, should make the needle deviate one degree.

The probes are of two kinds, those of which the construction is the more simple, are composed of two wires, one of copper[†], and the other of steel, soldered end to end, thus:—

Fig. 1.



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[*] The usurpation of the term *physician*, by the medical class in Britain, produces a great inconvenience. It obliges, in instances similar to the one in the text, the use of the vague and clumsy phrase of a *natural philosopher*. We would ask, respectfully, *could not* the reform which is working in the Royal College of Physicians go on and *label them correctly?*

[†] *Or platina.*

Each of them is about the $\frac{1}{30}$ part of an inch in diameter, and at least four inches long. One of these probes is to be introduced into that part of the body of which the temperature is wished to be known, taking care to place the soldered-junction in the middle of the part; then the communication is to be made between the two free ends of the probe and the extremities of the wire of the multiplier. These points of junction being placed in melting ice, in order that their temperature may remain constant; the magnetic-needle becomes deviated by reason of the difference of temperature which exists between that of the part under examination, and that of the ice. Now, as the current acts with so much the more power, as the angle of deviation is small, and experiment having proved that it is between 32° and about 77° that the maximum of effect is obtained, the base of the multiplier should be turned until the needle deviates from 68° to 77° before the commencement of the experiment, and then the current will be directed in such a manner, that the needle will retrograde towards 32°, and never pass further than from 77° to 86° in the other direction. In case it should exceed this, the current should be made to pass through a metallic wire so long that its intensity will be diminished sufficiently, and prevent a deviation exceeding the assigned limit. If these precautions are not taken, it would be impossible to observe minute differences in the intensity of the current, it being remembered, that the greater the deviation, the more obliquely does the current act upon the needle, and the less is this deviation augmented by an increase of intensity.

As soon as the magnetic needle is in a state of equilibrium, the probe is to be withdrawn from the part under examination, and its soldered-junction plunged into a basin of water, the temperature of which is to be raised until a deviation is produced of some degrees greater than that which had been before obtained. This water is then permitted to cool slowly, and the exact temperature corresponding to the observed deviation is to be ascertained by an excellent thermometer. This will be precisely that of the medium in which the soldered-junction was first inserted, the same thermo-electric effect being produced.

It is preferable, to determine the temperature by depression rather than by elevation, seeing that when the cooling is slow, it is more certain that the soldered-junction and the thermometer are evidently of the same temperature at the moment of observation.

In order that the cooling in the air of the non-immersed parts of the probe may not produce results below the true values, the free ends should be enveloped in linen, having the form of sheaths, (*a a*,) fig. 2. This precaution is not always enough, particularly when the temperature of the atmosphere is below 50°; in this case, the effect of the cooling *is very evident, hence* a neces-

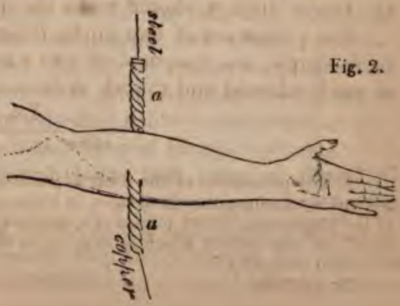


Fig. 2.

sity of operating as much as possible in an atmospheric temperature of at least 60°.

As the probe must be frequently detached from the wire of the multiplier, a mode of junction which admits of their easy union and separation should be adopted. The following expedient is that which has been found to be the most simple: the two ends of the wire (*b b*) of the multiplier are to be bent into spirals (*a a*), fig. 3, the circular apertures of which should be so small, that the extremities of the probes may be held after insertion with some degree of power. These apertures of the spirals should be frequently cleaned out with a fine bit of wood, and the ends of the probes should be rubbed, from time to time, with emery-paper, to remove any foreign substances which may adhere to their surfaces.

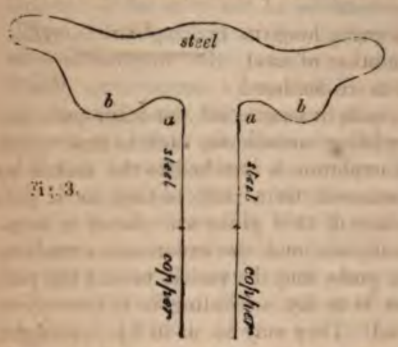


Fig. 3.

The method of experimenting which we have described, though very simple, yet requires the use of ice, which in a great many places may prevent its being followed; further, it gives results to about half a degree only, an approximation which is not sufficient in a large number of cases. This defect of sensibility arises from the too great difference between the temperatures of the two junctions; it is true, this might be reduced, by using two probes connected by an iron wire, as in fig. 3, and placing the two soldered-junctions in two different parts of the same animal; but the electro-chemical effects which would then result, might so disturb the results, as to lead persons not accustomed to distinguish thermo-electric effects, into error. It is also true, that the surfaces of the probes might be covered with several coats of lac-varnish, but the friction they undergo in their introduction is sufficient to remove it very soon, so that the first inconvenience soon occurs.

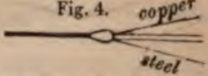
The electro-chemical effects may be prevented by placing one of the soldered-junctions in the mouth of an assistant, while the other is inserted into the medium which is the object of examination. The person who thus assists, should accustom himself to breathe entirely through the nose, in order that he may not introduce cool air into his mouth, and he should also be careful not to change the place of the soldered-junction. These two precautions are indispensable when a tolerably constant temperature is desired. As, however, the temperature of the mouth is perpetually varying, it is indispensable to examine occasionally with a thermometer which indicates tenths of a degree.

Attention should be paid that the wires should never be diminished in length, in order that equal deviations should always correspond to currents of equal intensity.

The form of the probes of the first kind requires that the animal *parts should* be perforated quite through, in order to disengage the two *extremities* which are to be placed in communication with the multiplier;

ut as there are cases where such perforations are not practicable, as for istance, when it is intended to determine the temperature of the esophagus, the stomach, the intestinal-tube; recourse must then he had another kind of probes, these have the form of the catheters which are sed in surgery, and of which the inspection of the following figure, 4, ill convey an idea.

Fig. 4.



Each probe is formed of two parts, longitu-inally, the one of copper* and the other of steel, oth terminating in a point, and there soldered gether for about one-tenth of an inch in length, all the other parts are parated by an insulating and resisting membrane, such as that which vers the back of a feather, this membrane is attached to the metals by elastic mastic, which can be renewed from time to time as it gets ose; the two disengaged extremities of this probe are placed in com-unication, as usual, with the multiplier, and the experiments made as ove described. The form of the probe may be varied to suit the pur-se for which it is intended, that is to say, according to the cavity or rt into which it is to be introduced. They may be, as in fig. 5, straight, curved, as fig. 6.

Fig. 5.

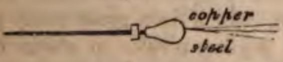
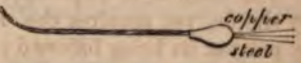


Fig. 6.



In probes of this construction, there is a risk that the membrane ay be torn in some part or other, and that, in consequence, the two ctions of the probe may communicate in other points besides the ldered ones. In order to ascertain whether these partial contacts exist not, the point of the probe is to be immersed in a quantity of water, the parts of which are ascertained to be of the same temperature, and e deviation of the magnetic-needle is to be observed; the probe-point is en to be lowered into the water for two or three inches; if the devia-n be not altered, it is certain that the two metals touch nowhere but their extremities: if it were otherwise, the deviation would vary.

Each time that different probes are used, it should be previously certained that they are entirely constructed of the same piece of metal, the smallest heterogeneity will influence thermo-electric effects. It ould also be remarked, that too much care cannot be taken in studying e movement of the multiplier when it is extremely sensible; without is, a risk is run of attributing to particular causes, effects which depend on local ones; for instance, when the magnetic-needle remains steady at ro, it would be fair to suppose that everything is symmetrical on each side, wever it is not always so; it may sometimes be observed that the needle brates further on one side than on the other by the action of the same rrent, according as it travels in one direction or another; this effect produced by the system of the magnetic needles being so perfectly atic, that it is affected by influences even at a distance; these tend to ect it more easily to one side than the other, according as they act by

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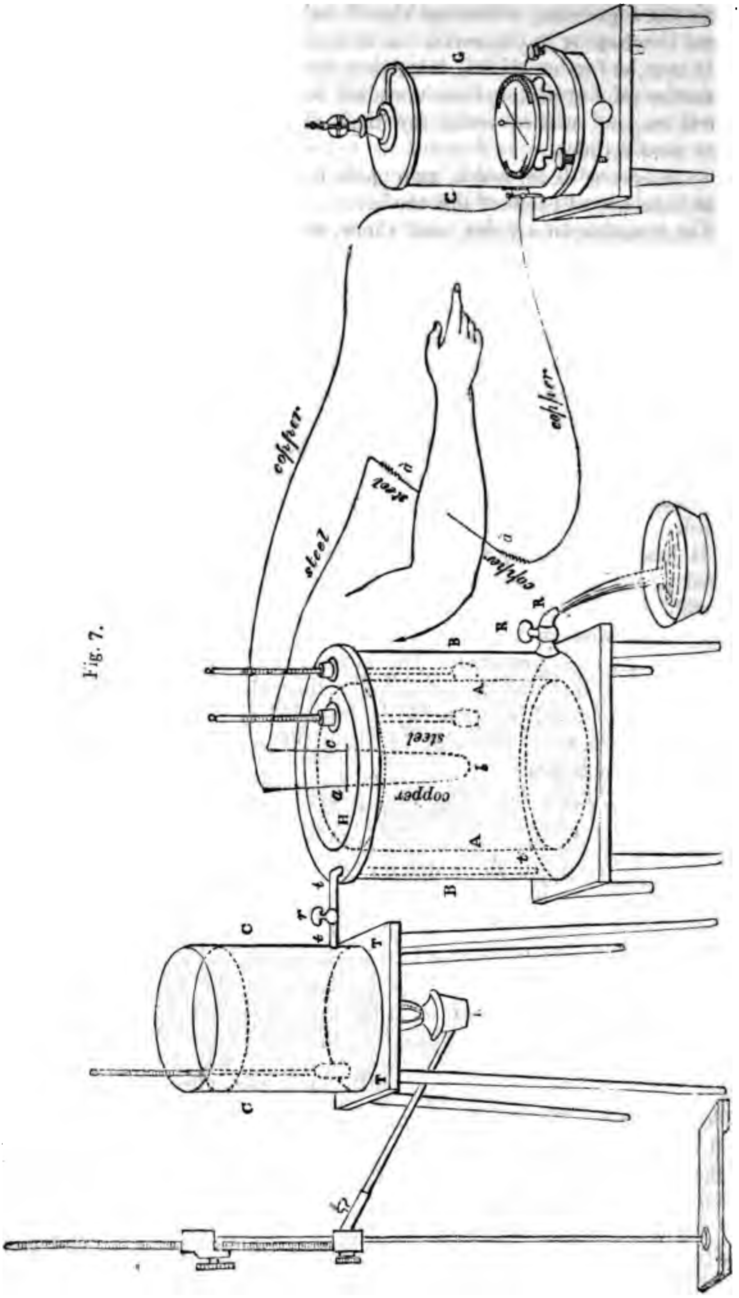
* Or of platina.

Fig. 7.


copper
steel
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steel
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copper
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ttraction or repulsion; advantage should be taken of this circumstance o direct the current in the manner most convenient.

It may be further added, that when the needle has deviated a certain number of degrees, and it is wished to observe minute changes of temperature, and consequently, by their thermo-electrical effects, the current should then be so directed as to lead the needle towards Zero, because it acts with so much more power, when its direction is, less oblique in relation to that of the needle.

The temperature of the mouth may, serve as a term of comparison in the absence of a better, but as frequent variations are always to be feared, depending upon the manner in which the soldered-junction is placed, this mode ought to be rejected whenever delicate examinations are entered upon. However, there is a means of verification, even in this case, which ought not to be omitted to be stated. The arrangement should be inverted, that is to say, that the soldered-junction of the second probe, or that which has been in contact with the mouth, should be placed in the part where the temperature is required. If the results are the same, their correctness is certain; if contrary, the cause of the difference must be sought after, and the experiment continued until an absolute equality is attained.

It has been found, after many attempts, that the apparatus delineated in fig. 7, has the advantage of conveniently affording a constant temperature.

A small wooden vessel (A), lined with sheet-lead, and having a wooden cover (H), is prepared. The cover has an aperture, by which a thermometer is introduced, and another through which is passed the probe, whose soldered-junction (*b*) is intended to be maintained at a constant temperature. This will be about 90° when mammalia are the subjects of examination.

Water at 122°, on being first poured into the vessel, suffers a depression of temperature, which can be permitted to any degree wished. The vessel is then to be placed in a receiver (B), whose height is rather greater, and into which water, heated to 104°, is to be poured. This temperature has been found to be of that height that the thermometer in the vessel (A) is not sensibly depressed when this falls a degree. The receiver is intended to impede the loss of heat in the vessel, and it is desirable that its arrangement should be such that the water in the receiver shall be maintained within a degree or so at the same temperature. Two means may each be employed for this purpose; one is to renew the heat in the receiver from time to time, by pouring in warmer water by means of a tube, removing at the same time an equal quantity of the water which has been cooled. But this operation is troublesome, and it may be substituted by the following, in which an apparatus regulates the introduction of the warming water, and the withdrawal of that which has been cooled.

On a stand (T) at a small distance from the receiver, is placed a vase (C) of tin plate; from this a copper tube (*t*) furnished with a cock (*r*) is carried into the receiver, and down to near its bottom. Another cock (R) is attached to the lower part of the receiver. Water is to be *maintained in the vase* (C) at a temperature of about 160°, by means of a lamp (L), its cock (*r*) is opened, and the hot water enters the receiver and is carried to its bottom; as this warmer water ascends in the receiver, it re-warms the whole apparatus; the receiver-cock (R) is now opened, and a proper quantity drawn off. With a little practice, and frequent observation of the thermometer, the constant temperature desired may be maintained in the vessel (A). One of the probes (*a b c*) is introduced into the vessel, and another (*a*' *a*') is inserted into any muscle whatever, and their ends connected and brought into communication with a multiplier (G). It will be necessary to construct previously a table of temperatures. Suppose it is proposed to operate upon the genus *Mammalia*, and that the temperature of one of the soldered-junctions is to be maintained at 97°, and the other plunged into a vessel of water, whose temperature can be varied from 86° to 112°. The deviation corresponding to each change of temperature must be noted, and when all the observations are tabularly arranged, the temperature corresponding to each deviation may be obtained on inspection.

With these means we may proceed to examine the temperature of the animal world.

[To be continued.]

REVIEW.

A Treatise on the principal Mathematical Instruments employed in Surveying, Levelling, and Astronomy: explaining their Construction, Adjustments, and Use, with Appendix and Tables. By F. W. SIMMS. Second Edition, improved and enlarged. 8vo., 128 pp., Cuts. London, Weale.

t would be difficult to find anywhere so much valuable information,
nveyed in so masterly a manner, as is done in the work before us.
Ir. F. Simms is peculiarly well qualified for his task, from his connexion
ith the partner of the late Mr. Troughton, who ranked as the first scien-
fic instrument-maker we have yet possessed,—from having been several
ears an assistant in our national observatory, and—from having formerly
een engaged in the Ordnance Survey of the kingdom. Mr. F. S. has
ius enjoyed unusual opportunities of studying all the instruments he
escribes in their most varied and improved forms; of using the astro-
omical ones under the superintendence of Mr. Pond, one of the best
bservers of his time; and of employing the geodæsical ones under the
iost eminent masters of the art of surveying. To such extraordinary
ieans of accurate acquaintance with his subject, he adds a power of
onveying his information in an elegant, because simple and unaffected,
ianner; and he has, consequently, produced a work invaluable to the
rofessional man, and interesting to every reader who possesses a taste
r practical science.

The title gives but an inadequate idea of the contents of the book;
r, in addition to a clear description of all the principal instruments used
ı surveying, and in astronomical observations, with an explanation
f their various adjustments, Mr. F. Simms has omitted little which has
ny reference to the subjects of surveying, levelling, and observing; he has
iven instructions for keeping the field-books, registering and reducing
bservations, with the formulæ for the latter, these he has *translated*,
s it were, into practical rules, for the sake of such learners as may not
ossess much mathematical knowledge; he has entered into details as to
ie mode of laying down, or *plotting*, a survey, and of copying or
educing plans, maps, &c., with a great variety of subsidiary matter; and
as furnished a small collection of valuable tables relating to these sub-
ects. In short, Mr. F. S. is one of those authors who, really anxious
instruct their readers, spare no pains which can facilitate their
rogress.

To enable our readers to judge for themselves of the merits we have
een commending, we select the following passage, describing the correc-
ons to be made for errors in the position of the portable transit instru-
ient:—

"From the description which has been given of the method of bringing a transit-instrument into a state of perfect adjustment, it might be inferred that it is essential it should be strictly so, to obtain accurate results from the use of it. It is certainly desirable that the adjustments should be examined and rectified as often as possible, as doing so ultimately saves the labour of computing the corrections to be applied to each observation, on account of the errors in the position of the instrument. But in some established observatories, where large instruments are employed, it is not attempted to put them in perfect adjustment, but the amount of the various derangements is ascertained from time to time, and the observations corrected accordingly. The adoption of this method, with so small an instrument as the one which we have been describing, where the adjustments are easily examined and corrected, will give indeed more accurate results, but, on account of the greater trouble, is not perhaps to be generally recommended; we shall, nevertheless, introduce in this place, an account of the method of computing these corrections, that persons possessing transit-instruments may adopt which method they think proper.

"The first correction is for the deviation of the line of collimation: the amount of the error may be determined by a micrometer attached to the eye-end of the telescope, by which, when the telescope is directed towards any distant object, the angular distance of that object from the central wire is measured in revolutions and parts of the micrometer-screw. The instrument is then reversed, and the distance of the same object from the central wire again measured, when half the difference of the measures is the error in collimation: and the angular value of a revolution of the screw being known, the corresponding value of the error is likewise known. The correction on account of this error to be applied to the time of each observation may be computed from the following formula.

$$\text{Correction} = \frac{c}{15} \text{ co-sec. } \pi$$

c = the error of collimation + if the deviation is toward the east.
π = (as before) the polar distance of the star.

"Hence we have in words this rule: To the log. of the deviation in collimation, add the log. co-secant of the polar distance of the star, and the arithmetical complement of the log. of 15: the sum will be the log. of the correction in time required.

"The next correction to be considered, is that arising from a want of horizontality in the axis. The spirit-level, which we described as striding across the instrument and resting on the pivots, determines the amount of the inclination of the axis, and also, as we have seen, enables the observer to correct it. Above the glass tube, and parallel to its length, is placed a fine graduated scale, the reading of which points out the number of seconds in arc that the pivots deviate from the true level, shown by the air-bubble receding from the centre towards that pivot which is the highest; but as it is necessary, when correcting for the adjustment, to remove half the error, by giving motion to the little screw on the level itself, so, for the same reason, in finding the measurement of the error, it is necessary to reverse the level on the axis, and read the scale at each extremity of the air-bubble in both its positions; that is, with the same end of the level on both the east and west pivots alternately,—and half the difference of the means of the two readings will be correctly the amount of deviation. This may be illustrated by the following example, in which the divisions on the scale represent seconds.

Readings of the Scale.

East End.		West End.
109″0		69″6
109,0		69,8
108,8		69,9
	Level Reversed.	
69,0		109,0
68,6		108,9
69,1		109,0
533,5	Sums	536,2
88,91	Means	89,37
		88,91
	Difference . . .	0,46

$\frac{1}{2}$ difference = 0,23 = the amount of deviation in arc, showing that the west end of the axis is higher by that quantity than the east end, since the mean of the western readings is greater than the mean of the eastern. This quantity, divided by 15, will give the proper factor for inclination. It is more convenient that the scale should be divided into units, each of which is 15″.

"Having in this manner determined the inclination of the axis by the level, the correction to be applied to the time of observation of any star made during the existence of that error, may be computed from the following formula:

$$\text{Correction} = b \cos. (\pi - \lambda) \text{ co-sec. } \pi.$$

b = the factor for inclination of the axis + if the west end be too high.
π = the polar distance of the star.
λ = the co-latitude of the place.

"This formula in words gives the following practical rule. To the log. of the factor for inclination of the axis, add the log. co-secant of the polar distance, and the log. co-sine of the difference between the polar distance and the co-latitude: the sum − 20 will be the log. of the correction in time required.

Convinced as we are that a third edition will soon be called for, we would suggest the following additions to the author's consideration.

A general preliminary essay on the causes of derangement in astronomical instruments, and the nature of the adjustments which are hence rendered necessary, whenever the instrument is used. These causes of derangement are principally the unavoidable imperfection of workmanship, the wear of the centres by friction, and the effects of temperature and gravitation in altering the form of the divided limbs of graduated arcs.

We think some details of the method of observing and noting the times, would be both useful and interesting—thus, the observer having written down the integral minute when he takes his place at the telescope then counts the seconds'-beat, of his clock, and notes them at the passage of the star over each wire of the micrometer. The technical mode of "reading off" the *micrometers*, when several are attached to the same *circle, also seems to de*mand some description.

A less concise notice of the method of obtaining the longitude by the eclipses of Jupiter's satellites, in p. 102, is very desirable, from its importance; it is at present far too slightly touched on.

There are one or two instruments not noticed which would seem to require description in such a work, and some are, perhaps, too briefly alluded to; Captain Kater's collimator, and the *repeating-table*, p. 19, may be cited as instances of the two cases alluded to.

A glossary of the technical and scientific terms made use of would be a useful addition; thus, for example, in p. 29, the term "*sprung* at its aperture," would not be intelligible to one less conversant than ourselves with instruments; and the term *collimation*, might be dwelt on more at large, considering its importance and frequent recurrence.

We make these suggestions from a conviction that they are *desiderata*, and that few persons could treat them better than our author; otherwise, where so much has been done so well, it might seem invidious to introduce even these hints.

One omission we feel we ought to supply, as it happens to be in our power. We do it the more readily, because the improvement in question having been attributed to, if not claimed by, another, it becomes an act of justice to assign it to the inventor. In page 57, is mentioned "an excellent contrivance for taking altitudes and depressions with the box-sextant," which was shown to the author by a professional gentleman. This gentleman, we believe, was Mr. Macneill, the civil-engineer; we are sure that he was the inventor of this modification of the instrument. Subsequent experience has decided that the new power is a very useful one. Several practical civil-engineers of eminence have so expressed themselves with regard to it, and now consider a box-sextant so provided as the most convenient instrument that can be taken, when an accurate *coup-d'œil* is to be made of a new line of country. It has been found that one of the two spirit-levels mentioned in the text, may be dispensed with, and that even when this is done, a very little practice enables an observer to take an angle of altitude within 5 sec. of the truth*.

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* In the paragraph of the work describing this addition to the box-sextant, for "plane-sight" read "plain-sight."

MISCELLANEOUS INTELLIGENCE.

Precision in Scientific Terms.

CONDUCTION—CONVECTION.—"The transfer of heat by communication may be distinguished into transfer by *conduction*, and transfer by *convection*. The former being applied to the method of transfer which takes place in solid bodies, and the latter in fluids.

When a solid mass is placed in contact with a hot substance, the heat is communicated from one to the other, and is transmitted through the solid, being conducted from one particle to another, with a velocity which depends on the nature of the substance in contact; hence substances have been divided into good and bad conductors.

But when heat passes by communication from a hot body into fluids, the transfer takes place in a very different manner. The particles of a fluid, becoming heated, expand, and then being specifically lighter than those particles of the surrounding fluid which have not received an increase of temperature, they ascend, and fresh particles descend into their places; and the transfer of heat by this motion is so rapid, that if one thermometer be placed at the top and another at the bottom of a vessel of fluid which is heated from below, the upper thermometer will begin to rise almost as soon as the lower: this, then, is the transfer by *convection*.

The *conducting* power of fluids is extremely small. For if heat be applied to the upper surface of a fluid, the *convecting* motion of the particles cannot take place, and consequently, the only transfer of heat which can take place, will be by conduction, in consequence of the contact which subsists between the particles; and the transfer which takes place this way is so small, that Count Rumford denied that water could *conduct* at all. But it appears that all fluids conduct heat in a slight degree. Hence the transfer by communication, that is, by conduction and convection, depends upon the nature of the substance.

The term *convection*, which was much wanted, is introduced on the authority of Dr. Prout, *Bridgewater Treatise on Meteorology, &c.*"—Webster, *Principles of Hydrostatics*, 1835.

CLIVITY—ACCLIVITY—DECLIVITY.—"I have rendered the word *pente* by slope, in preference to *inclination, inclined plane*, or *gradient*, considering the two former, though generally used, as improper expressions, and the latter, to say the least of it, as having so very little to recommend it, that I hope it will have an extremely short existence in our nomenclature.

Judicious and appropriate terms are of the greatest importance in speaking and writing on scientific subjects, particularly when technical expressions must of necessity be introduced. "It is highly desirable to keep scientific knowledge precise, and always to use the same terms in the same sense[*]."

A gentleman of high literary acquirements, to whom I applied, and who has taken the trouble to consider the subject, has suggested the term *clivity*, as one that is of more legitimate etymology than *gradient*, and more appropriate than either *slope, inclined plane*, or *inclination*. I regret that I was not in possession of this term before I commenced the translation; the words

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[*] *Lord Brougham's Discourse on Natural Theology.*

acclivity, declivity, which may be so regularly derived from it, would have enabled me to have given the sense of the original with greater perspicuity."—Macneill, *Translation of Navier on Railways*, 1836.

Remarkable Depression of the Barometer.

The barometer at the Royal Observatory, Greenwich, at noon on the 28th ult. stood at 28·525 in. A depression which has not been observed at this season for more than thirty years. The day was rainy, and there was some wind at night: nothing more remarkable was observed.

Professor Daniell gives the following as the barometrical points of March in London:—

	Inches.
Greatest height . . .	30·770
Mean height	29·843
Least height	28·870.

The depression on the 28th ult. was therefore lower than the least height here given, by 0·345 in.

Recent Hypothesis on the Formation of Rain not new.

In your *Magazine of Popular Science* for February, p. 23, there is an hypothesis stated to be given by Professor Phillips, of King's College, in explanation of the difference in the relative quantities of rain collected at different heights. Professor Bache of Pennsylvania, in a letter communicated to the Franklin Institute Journal, dated Dec. 1835, observes:—" In investigating a complex subject of this kind, the experimenter not unfrequently proceeds as if it were entirely new, and to this cause I attribute the fact, that Professor Phillips was not aware that he had been anticipated in his hypothesis. The fact that a less quantity of rain is received by a rain-gauge upon an elevation than upon the subjacent ground, was proved about the year 1766, by the experiments of Dr. Heberden, Lord Cavendish, and others*. The hypothesis now advanced by Professor Phillips, was suggested about 1771, as an explanation of their curious results by Dr. Franklin, and he is not the less entitled to the credit of originating it, that after fully considering the subject, he cautiously concluded, that the then state of knowledge of it was hardly ripe for making *any hypothesis*. 'I think we want more and a greater variety of experiments in different circumstances, to enable us to form a thoroughly satisfactory hypothesis.' The demonstration of the hypothesis, if it is considered conclusive, is sufficient distinction, and belongs to a more advanced state of science than the eighteenth century could boast. The credit of *originating it* we should abandon.

"I have not the least doubt that when these observations of Professor Bache, reach the eye of Professor Phillips, it will give him little or no uneasiness, to find that such an accurate observer as Dr. Franklin had preceded him in an hypothesis. The Doctor's remarks were continued in a letter to Dr. Percival, who has also written on the subject, and first published in the Manchester Memoirs for 1784."—*Extract from a Letter to the Editor, April*, 1836.

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* There is a curious anachronism committed by the author of the article in the *Magazine of Popular Science*, entitled "Recent Researches on the Formation of Rain." He states that the experiments of Dr. Heberden on this subject confirm the *recent ones* of M. Arago, whereas Dr. H's experiments must have been made before *the latter philosopher* was born!

Deserving Pensioners.

If the word "Pension" be correctly defined to be "an allowance made to any one without an equivalent,[*]" it is high time another was substituted, to designate that reward which a nation decrees for services already performed; and also that the following names, so triumphantly and properly introduced by Mr. Spring Rice, the Chancellor of the Exchequer, in his recent speech on the "Pension List," should at least be erased from that catalogue, and placed on a very different roll.

On Mr. Whittle Harvey having challenged the government to produce ten names of intellectual merit on the Pension List, the Chancellor of the Exchequer replied:—"I have been challenged to show ten names on the Pension List that have been distinguished for their public services, or for eminence in literature, in science, or in any other way that could possibly be a recommendation for their being placed on that list. Now I accept that challenge. The list I am about to give contains the names of

Dr. Dalton,
Mr. Ivory,
Mr. Airy,
Mrs. Somerville,
Dr. Southey,
Mr. Montgomery,
Mr. Sharon Turner,
Sir James South,
Mr. Thomas Moore,
Professor Faraday

and, if I chose, I could go on and double the List."

It is so rarely that science or any of her votaries, receive attention from an English minister, that it is with new and grateful feelings that we give the above extract, we must also observe that it is curious to see how quickly the politician was aware of the advantage of his position, in being prepared to quote even so small a number of really-deserving pensioners. We have great hopes that his sagacity will early induce him to strengthen it still more.

Nebulæ.

"The discoveries as to the nature of the bodies of our solar system, which the use of the telescope presented to its first inventors, must have been unexpected, and in no small degree astonishing. Yet we may safely assert, that they exhibit no remarkable novelty. The ring of *Saturn* alone can be considered as materially different from the objects constantly before our eyes. In all the other planets we see bodies similar in shape to our earth, analogous (as we have good reason to believe) in internal constitution, revolving in like manner, enlightened by the same principal luminary, and by similar satellites, the subjects and the exciters of similar attractions, and possessing at least some similarity in the construction of their surfaces, and in the phenomena of their atmospheres, as far as spots and belts enable us to conjecture. Everything, in fact, leads us to conclude that they are bodies of the same order: that, with specific differences, there is a generic resemblance; that the circumstances of formation, which have bound all in one mechanical system respecting the sun as the principal seat of force, have also impressed upon all one physical system, as testified by the similar arrangement of subordinate bodies, and the probable resemblance of their gaseous as well as their solid parts.

But, when we look into the sidereal world, the analogy of system fails entirely. A star, it is true, may be conceived analogous to our sun: a double star, forming a binary system, though we have nothing here exactly like it, is still not remarkably different in its nature from a single one: but a star, regularly surrounded by dense nebulous matter, an irregular nebula, in which one point is brighter than the rest, a nebula in which all idea of a stellar point is lost,—all these present instances of appearance, gradually yet totally different, and entirely dissimilar from every permanent body in our system. The resolvibility of some of these nebulæ, implying the existence of an immense number of stars at a proximity apparently much greater, in proportion to their individual brightness, than the stars which we commonly see, is a very striking phenomenon; but far more striking is the irresolvibility of others, whose magnitude seems to imply comparative nearness, which, nevertheless, defy our telescopes, and whose general appearance seems obviously to contradict the notion of consisting of groups of stars. Among the most remarkable of these, I may mention the two most conspicuous—those of *Andromeda* and *Orion*. No one, I think, who has seen these in a telescope of great light,—the one like a lamp shining through a homogeneous fog, the other like a pile of cumuli-clouds, tossed together in the same capricious manner in which we see them in our summer-skies,—can persuade himself that these can be anything but masses of nebulous matter, the causes and the laws of whose arrangement we should vainly endeavour to detect.

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[*] Walker's Dictionary, edit. 1822.

In these remarks I have alluded only to the difference between the present appearance of these bodies, and that of the planets depending on our sun. We may now, however, consider the matter in another point of view. The phenomena of the solar system impress upon us the notion, not only of similarity, but of contemporaneity; at least, they seem to inform us that the time which has elapsed since the states of the planets were sensibly different must be immensely greater than the time during which a gradation of formation could have been sensible. But the contemplation of different nebulæ suggests a new idea—the idea of change. In one, we find nebulous matter in the wildest confusion; in another, there are spots in which, apparently, a concentration of the matter has been formed by drawing together the nebula from a large space, and leaving the neighbourhood comparatively dark (an effect exhibited in such various ways, that it is impossible to consider it as an optical illusion, the effect of contrast): in others, we have rings of nebulous matter enclosing a dark space. A more common case is the concentration which, in various degrees, exhibits the various appearances of planetary nebulæ and nebulous stars. And one very curious instance has been pointed out in which the segregation has taken place in a honeycomb form, the lines of the honeycomb being nearly accompanied by lines of stars. But, has astronomy yet observed any change in these bodies? We cannot say with certainty that it has; yet the notion of change is not the less impressed upon us. To use the powerful illustration of Laplace, we look among them as among the trees of a forest. The change during the interval of a glance is undiscoverable, yet we perceive that there are plants in all different stages. We see that these stages are probably related to each other in the order of time; and we are irresistibly led to the conclusion, that the vegetable world in one case, and the sidereal world in the other, exhibit to us, at one instant, a succession of changes requiring time, which the life of man, or the duration of a solar system, are alone sufficient to trace out in any one instance.

Let it not be thought that the telescopic minuteness of some of these bodies is any argument against the importance of the investigation into their nature. The question as to the annual parallax of any nebula, has hardly, perhaps, received sufficient attention; and its practical determination must necessarily be embarrassed with difficulties. This only we can assert, that the parallax of those most frequently observed is not conspicuous, and, probably, is not sensible. If the parallax of the great nebula of *Orion* be no greater than that of the stars most carefully observed, the breadth of that nebula may be fifty or a hundred times as great as the diameter of the earth's orbit. It may, then, well contain a sufficiency of matter for the formation of a sun and a system of planets. With this consideration, the examination of nebulæ acquires a new interest. It is not merely the inspection of a series of natural changes, in which we have no greater interest than in the transitions from an egg to a moth; but it is the study of the successive steps by which worlds like that which we inhabit, and that which regulates our motions and our seasons, may have been organized from the most chaotic of all conceivable states. When to this we add, that the combination of relative motion of parts with gradual concentration of mass is sufficient to account generally for the formation of planets and satellites, possessing that remarkable property which is possessed by the bodies of our system, of revolving all in the same direction, and describing orbits nearly circular, we must acknowledge that the examination of nebulæ, in all their stages, presents not merely a chance, but a highly-plausible chance, of forming a distinct theory of cosmogony. And if we admire the genius of the mighty mathematician who first pointed out the simple reasoning by which the transition from nebulous fluid to discrete planets may be shown to be physically possible and probable, let us at the same time pay our tribute of admiration to the great astronomer whose accurate observations, and sagacious reflections, gave the first ground for such a theory. Little time has elapsed since the first observation of these sidereal bodies: the observations of the greatest part of them have been made within our lifetimes: the first page in that part of the history of astronomy which relates to these subjects is hardly yet traced. But the history of astronomy may yet be long enough to comprehend a series of visible changes; and the most important element for the value of that particular branch of it will be the fulness and accuracy of the commencement. Happy would it be for other parts of the science, if the first pages of their history were as well traced."—*Astron. Soc. Report*, 1836.

Uncertainty of the Signs of Death.

In the *Clinique Chirurgiale* of M. Larrey, the case of a French officer is stated, who, though still living, has been already twice interred

Weather at Brussels.

The climate of Brussels is far from being attractive, M. Quetelet gives the following results from observations made at the Observatory there, during the last three years.

		1833.	1834.	1835.
Number of days of	Rain . . .	180	166	161
"	Hail . . .	5	8	12
"	Snow . .	11	8	12
"	Frost . . .	39	21	46
"	Thunder . . .	7	13	5
"	Mist	25	19	25
"	Sky, entirely cloudy .	48	27	42
"	Sky, entirely clear .	12	30	13

New Milling-Press, and Assay-Weights in the United States' Mint.

MR. Franklin Peale is about to introduce a new Press for milling coins into the mint of the United States. This gentleman has recently returned from a mission, undertaken at the expense of his government, for the purpose of examining the assaying and coining processes of Europe.

It is, we believe, also, on the recommendation of Mr. F. P., that the decimal assay-weights of the French are about to be introduced into the assaying department of the same establishment, instead of the miniature ounce, and its divisions, of the English assayers, which have hitherto been used in the United States. The simplicity which this change will produce in the laboratory calculations, we can understand; but unless Great Britain takes a similar measure, we suspect it must be attended with some commercial inconvenience. It is, however, a step in advance; for though the decimal division is far from being satisfactory in all cases, it is very superior to our complicated system, which has two kinds of ounces, and divides one of them into twenty parts, and each of these subdivisions into twenty-four more Thus setting at defiance all correct mental estimates of proportion on the inspection of numerical quantity.

Royal Astronomical Society, February, 1836.

THE number of the existing Fellows of the Royal Astronomical Society of London, after deducting defaulters, was, in February last, . 287
Ditto of Associates 37
Total 324

His Majesty Frederick VI., King of Denmark, has been unanimously elected an Honorary Member of this Society, as a testimony of the Society's gratitude for His Majesty's beneficent intentions and useful services with regard to astronomy and astronomers.

New Meteorological Observatory.

IN the Annual Report made on the Anniversary of the United Service Museum, in March last, is the following passage.

"It is proposed to keep a Meteorological Journal at the Museum, for which purpose necessary instruments will be provided, and the results noted."

The utmost care should be taken that these "necessary instruments" are of the most accurate construction, and made after approved models, so that the "results noted" may be compared with others. It appears to us from the number of meteorological observers of the present day, and their dispersion over nearly the whole globe, that it is time there was a general understanding between them as to the hours, instruments, registry, &c. of observations. At present, an immense quantity of valuable time is thrown away in making and recording perhaps excellent observations, but on which no confidence can be placed, because the circumstances under which they are made are not known.

Patents in 1835.

THE total number of grants made for Patents in England, in 1835, was 231
" " Scotland, . . . 92
" " Ireland, (unknown).
The total number of Patents issued from the Patent-office of the *United States* of America, in the same year, was 772

Planetary Ephemeris.

THE first idea of this work, so useful in a working observatory, was due to Mr. Sheepshanks, who calculated and printed the first, at his own expense, in 1830. The expense, &c., up to the present year inclusive, was afterwards borne successively by the Astronomical Society, and by Mr. Baily. Upon a representation to the Government, by the Society, of the importance and public character of such a work, and that its utility had been verified by experience, it is gratifying to be able to state that prompt attention was immediately paid to it by the Lords of the Admiralty, and a communication made by them to the Society, that in future its publication would be undertaken and paid for by the Government.

Railroad Acts.—Present Session.—(April 24th, incl.)

THE first Railroad Bill which has passed into an Act of Parliament this Session, is the Birmingham and Gloucester. It received the Royal Assent on the 22nd. No other is ready for it; but the following have passed the House of Commons, and are in progress in the House of Lords, viz.

Arbroath and Forfar,
Great Western Amendment,
Hull and Selby,
Ulster,
Dundee and Arbroath,
Bristol and Exeter,
Sheffield and Rotherham,
Cheltenham and Great Western,
Bolton and Leigh,
London Grand Junction.

The Birmingham and Derby has passed both Houses, but having received an amendment in the Lords, it is returned to the Commons for their consideration.

Softening effect of Water on Cast-Iron.

SOME large brass and cast-iron guns, which went down with the Royal George in 1782, are now lying in the the Tower. The brass ones are little affected by their long immersion in the sea; but those of cast-iron are changed throughout their whole substance. They resemble plumbago or pencil-lead, and, like it, may be easily cut with a knife.

Cast-iron pipes, attached to a pumping-apparatus, in a mine, 140 fathoms deep, in the north of England, have been so softened in five years, as scarcely to hold together on removal.

Tides in the Western Hemisphere.

AN extensive series of Tide Observations was made along the Atlantic coast of the United States, in June last, under the direction of the commander-in-chief of the United States' army, in consequence of a request made to the executive of the United States, by the government of Great Britain. Some of the journals ought, perhaps, before this, to have contained, at least, a general account of these observations.——*Journ. Franklin Institute.*

United Service Museum.

THE establishment of the United Service Museum, in Scotland Yard, is rapidly advancing in extent and utility.

The number of Subscribers on the 5th of March last, was . . . 4193
The number of Visiters to the Museum during the year 1835, was 8537

Pension to Mr. Peter Nicholson.

A PETITION to His Majesty is now in the course of signature in the metropolis, requesting the grant of a Pension to Mr. Peter Nicholson. A more deserving object of the Royal and National Bounty was never recommended to its attention. After a long and eminently-useful life, this teacher of practical science, whose works have enriched others, and have extended, and will continue for ages to extend, the application of science to some of the most necessary and valuable arts of life, is now, at an advanced age, depending upon a subscription which his private friends of Newcastle and its neighbourhood have raised, and which, though not yet exhausted, is daily approaching to its end. Without intending to excite any invidious comparison, we are ready to maintain and to prove, that as a citizen deserving distinction and aid, for useful intellectual labour rendered to his country, Mr. Nicholson's claim is as strong as any one on that list, so honourable to the government, which the Chancellor of the Exchequer lately gave in the House of Commons. In one respect, it has unhappily an additional title to attention. *Mr. Nicholson is in want.*

Astronomical Society's Medal for 1836.

THE Catalogue of Nebulæ and Clusters of Stars, made by Sir J. F. W. Herschel, and published in the *Philosophical Transactions* for 1833, has been decided to be worthy of the Gold Medal of the Astronomical Society; and was on the 12th of February last publicly consigned to the care of Captain W. H. Smyth, Foreign Secretary, by the President of the Society, in the following terms:—"Captain Smyth,—Transmit this Medal, in the name of the Royal Astronomical Society, to Sir John Herschel. Assure him that we admire the genius and appreciate the perseverance which have produced the Catalogue of Nebulæ and Clusters of Stars,—that we respect the motive which has prompted him to establish himself for a time at a distance from his country,—and that we join in the warmest wishes that his residence there may be one of enjoyment and satisfaction, and that his return may be happy and honourable."

Fire-proof Chest.

A WOODEN Chest, lined inside and outside with asbestos, and then enclosed in a stout sheet-iron case, has been constructed as a Fire-proof Chest, by Mr. Scott of Philadelphia. An instance of its preservative power occurred during the recent immense fire at New York.

Opposing Opinions on Radiation.

"DR. Stark of Edinburgh has endeavoured to show, that black substances radiate better than white ones. He has made a few experiments, directly applicable to the subject, which, as far as they go, warrant this conclusion."

"Professor Bache of Pennsylvania, has made an extensive series of experiments on a similar method to that of Dr. Stark, the result of which is, that colour does not appear to influence the radiation of heat, unaccompanied by light."—*Journ. Franklin Institute.*

Patent Law Grievance.—No. 2.

UP to the 26th ult. has the Government taken, in the present year only, out of the pockets of Inventors, for stamps and fees in the passing of Patents, in Great Britain, very considerably more than £15,000. We hope to be able to state that Mr. Mackinnon, on the 28th, reported this heavy grievance to the *House of Commons.* To what purpose are all these thousands applied?

NEW PATENTS. 1836.

ENGLISH.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

GRANTS.

MARCH *contd.*

73. William Gossage, Stoke Prior, *Worc.*, Chemist, and Edward White Benson, Wichbold, *Worc.*, Chemist; for improvements in the process of making or manufacturing ceruse or white-lead. Mar. 29.—Sep. 29.

74. James Noble the Elder, Mill-place, Commercial-road, *Middx.*, Wool-comber; for certain improvements in the combing of wool and other fibrous substances. Mar. 29.—Sep. 29.

75. Charles de Bergue, Clapham-rise, *Surr.*, Engineer; for certain improvements in machinery used for spinning and doubling yarn or thread, manufactured from cotton or other fibrous material. Mar. 29.—Sep. 29.

76. William Brindley, Caroline-st., Birmingham, *Warw.*, Paper-maker; for improvements in the manufacture of tea-trays, and other japanned ware, and in the board or material used therein, and for other purposes. Mar. 29.—Sep. 29.

77. Thomas Cockerell Hogan, Castle-st., Holborn, *Middx.*, Light Hat-manufacturer; for certain improvements in hats, caps, and bonnets. Mar. 29.—Sep. 29.

78. Andrew Parkinson, Low Moor, *Lanc.*, Overlooker of power-looms; for an improved stretcher to be used in, or with hand- or power-looms. Mar. 29. Sep. 29. *For. Comm.*

79. Samuel Parlour, Addiscombe-road, Croydon, *Surr.*, Gent.; for certain improvements applicable to sketching, drawing, or delineating. Mar. 31.—Sep. 30.

Total, March...36.

APRIL.

80. John Jeremiah Rubery, Birmingham, *Warw.*, Umbrella and Parasol-furniture manufacturer; for certain improvements in the making or manufacturing umbrella and parasol stretches. April. 7.—Oct. 7.

81. John Spurgin, Guilford-st., Russell-sq., *Middx.*, M.D.; *for a new or improved ladder, or machinery* applicable to the working of mines and other useful purposes. Apr. 7.—Oct. 7.

82. John Holmes, Birmingham, *Warw.*, Engineer; for certain improvements in the construction of boilers for steam-engines. Apr. 7.—Oct. 7.

83. Thomas Ridgway Bridson, Great Bolton, *Lanc.*, Bleacher; for certain improvements to facilitate and expedite the bleaching of linen and other vegetable fibres. Apr. 7.—Oct. 7.

84. Robert Copland, Brunswick-crescent, Camberwell, *Surr.*, Esq.; for improvements upon patents already obtained by him for combinations of apparatus for gaining power. Apr. 9.—Sep. 9.

85. Miles Berry, Chancery-lane, *Middx.*, Civil-engineer; for new or improved apparatus or mechanism for marking down or registering the notes played on the keys of piano-fortes, organs, or such other keyed musical instruments. Apr. 12.—Oct. 12. *For. Comm.*

86. Jacob Perkins, Fleet-st., *Lond.*, Engineer; for certain improvements in steam-engines, and in generating steam, and evaporating and boiling fluids for certain purposes. Apr. 12.—Oct. 12.

87. James Leman, Lincoln's-inn-Fields, *Middx.*, Gent.; for improvements in making and manufacturing soap. Apr. 12.—Oct 12. *For. Comm.*

88. Thomas Hodgson Leighton, Blyth, *Northumb.*, Chemist; for certain improvements in the converting sulphate of soda into the sub-carbonate of soda, or mineral alkali. Apr. 12.—Oct. 12.

89. Joshua Bates, Bishopsgate-st., *Lond.*, Merchant; for certain improvements in machinery for cleaning and preparing wool. Apr. 16.—Oct. 16. *For. Comm.*

90. John Parkinson, Rose Bank, Bury, *Lanc.*, Calico-printer; for certain improvements in the art of block printing. Apr. 19.—Oct. 19.

91. James Pedder, Radford, *Nott.*, Lace-maker; for certain improvements in certain machinery, for making by means of such improvements, figured or ornamented bobbin-net-lace. Apr. 21.—Oct. 21.

92. Henry William Nunn, Newport, *Isle of Wight*, Lace-manufacturer; for certain improvements in manufacturing or producing certain kinds of embroidered lace, parts of which improvements are applicable to other purposes. Apr. 21.—Oct 21.

93. Hamer Stansfield, Leeds, *York*, Merchant; for machinery for a method of generating power applicable to various useful purposes. Apr. 23.—Oct. 23. *For. Comm.*

94. Edward John Dent, Strand, *Middx.*, Chronometer maker; for an improvement of the balance-springs, and their adjustments of chronometers and other time-keepers. Apr. 23.—Oct. 23.

95. James Findon, Black Horse Yard, High Holborn, *Middx.*, Coach-smith; for improvements in apparatus for supplying water to water-closets. Apr. 23.—Oct. 23.

96. George Augustus Kollman, Organist; for improvements in railways and in locomotive carriages. Apr. 23.—Oct. 23.

97. Edward John Massey, Liverpool, *Lanc.*, Watch-maker; for improvements in railway and other locomotive carriages. Apr. 23.—Oct. 23.

98. Sampson Mordan, Castle-st. Finsbury-sq., *Middx.*, Mechanist; for an improvement in making or manufacturing triple-pointed pens. Apr. 23.—Oct. 23.

99. William Taylor, Smethwick, *Staff.*, Engineer, and Henry Davies, Stoke Prior, *Worc.*, Engineer; for certain improvements in machinery or apparatus for introducing water or other fluids into steam-boilers, or evaporating vessels, also for obtaining mechanical power by the aid of steam, and for communicating motion to vessels floating in water. Apr. 25.—Sep. 25.

100. Thomas Aitken, Edenfield, Bury, Spinner and Manufacturer; for certain improvements in the preparation of cotton and other fibrous substances, and in the conveyance of the same to roving-frames, mules, throstles, or any other spinning or doubling machinery. Apr. 25.—Sep. 25.

ENROLMENTS of GRANTS in 1836.

28. Kyan, Prevention of Vegetable Decay. March 17.

SCOTCH.

N.B. The number in ( ) is that of the English Patent in this Magazine granted for the same Invention

GRANTS.

JANUARY.

1. Elijah Galloway, Westmoreland-place, City-road, *Middx.*, Engineer; for certain improvements in steam-engines, which improvements are applicable to other purposes. Jan. 8.—May 8.

2. James Bullough, Blackburn, *Lanc.*, Mechanic; for certain improvements in hand-looms and power-looms. Jan. 8.—May 8.

3. John Malam, Kingston-upon-Hull, *York*, Civil-engineer; for certain improvements in gas-meters, and in the apparatus for generating gas for illumination. Jan. 11.—May 11.

4. Joseph Whitworth, Manchester, *Lanc.*, Engineer; for certain improvements in machinery for spinning, twisting, and doubling cotton, flax, wool, and other fibrous substances. Jan. 14.—May 14.

5. (3.) William Harter, Manchester, *Silk-manufacturer*; for certain improvements in machinery for winding, cleaning, drawing, and doubling hard and soft silk; which improvements are also applicable to machinery for winding, cleaning, and doubling thread or yarn manufactured from cotton or other fibrous materials. Jan. 15.—May 15.

6. Thomas Jevons, Liverpool, *Lanc.*; for certain improved machinery to be used in manufacturing bars or wrought-iron into shoes for horses, and also into shapes for other purposes. *For. Comm.* Jan. 15.—May 15.

7. Thomas Greig, Rose Bank, Bury, *Lanc.*, Calico-printer; for a mode of embossing and printing, at one and the same time, by means of a cylinder or roller, on goods or fabrics made of or from cotton, silk, flax, hemp, and wool, or any one or more of those materials, or on paper. Jan. 18.—May 18.

8. Andrew Smith, Princes-st., Haymarket, *Middx.*, Engineer; for a new standing rigging for ships and vessels, and a new method of fitting and using it. Jan. 19.—May 19.

9. John Day, York-terrace, Peckham, *Surr.*, Gent.; for an improved wheel for carriages of different descriptions. Jan. 20.—May 20.

Total, January...9.

February.

10. (8.) Moses Poole, Patent Office, *Middx.*, Gent.; for improvements in Jacquard looms. Feb. 1.—June 1. *For. Comm.*

11. John Cooper Douglas, Great Ormond St., *Middx.*, Esq.; for certain improvements in making vinegar from various materials, and in making useful articles from the refuse of such materials, and also in apparatus for applying and conducting heat to liquids to be used in the manufacture of vinegar and other purposes. Feb. 1.—June 1.

12. Lightly Simpson, Manchester, *Lanc.*, Alchymist; for certain improvements in the preparation of certain colours to be used for printing cotton and other fabrics. Feb. 3.—June 3.

13. John George Bodmer, Bolton-le-Moors, *Lanc.*, Engineer; for certain improvements in machinery for preparing, roving, and spinning cotton and wool. Feb. 3.—June 3.

14. James Brown, Esk Mill, Pennycuick, *Edinb.*, N. B.; for certain improvements in the making or manufacturing of paper. Feb. 4.—June 4.

15. John Hewitt, Kinezie, *Cornw.*, Gent.; for a combination of certain materials or matters, which being combined or mixed together, will form a valuable substance or compound, and may be used with or as a substitute for soap. Feb. 4.—June 4.

16. James Kean, Johnston, *Renfr.*, N.B., Machine-maker and Engineer; for an improved throstle flyer, or a substitute for an ordinary flyer, employed in spinning cotton, flax, hemp, wool, silk, and other fibrous substances. Feb. 12.—June 12.

17. (23.) Edmund Ashworth, Egerton, *Lanc.*, Cotton-spinner, and James Greenough, of the same place, Overlooker; for certain improvements in the machinery used in preparing and spinning cotton, silk, wool, and other fibrous materials. Feb. 18.—June 18.

18. (10.) Franz Moll, Grove Lane Terrace, Camberwell, *Surr.*, Esq.; for improvements in preserving certain vegetable substances from decay. Feb. 18.—June 18.

19. (15.) Julius Jeffreys, Osnaburgh-st., Regent's Park, *Middx.*, Esq.; for improvements in curing or relieving disorders of the lungs. Feb. 18.—June 18.

20. (19.) William Boulnois, Jun., Gower-st., *Middx.*, Esq.; for an improved combination or arrangement of springs for carriages. Feb. 27.—June 27.

21. Robert Griffith, Birmingham, *Warwick*, Machine-maker; for improvements in machinery for making rivetts, screw-blanks, and bolts. Feb. 27.—June 27.

Total, February...12.

March.

22. William Wainwright Potts, Burslem, *Staff.*, China and Earthenware manufacturer; for an improved method or process of producing patterns in one or more colours, to be transferred to earthenware, porcelain, china, glass, and other similar substances. Mar. 1.—July 1.

23. John Baillie, Great Suffolk-st., Southwark, *Surr.*, Engineer; for improvements in propelling of vessels and other floating bodies, by means of steam or other power. Mar. 1.—July 1.

24. Miles Berry, Chancery-lane, *Middx.*, Civil-engineer; for certain improvements in power-looms for weaving. Mar. 4.—July 4. *For. Comm.*

25. William Wilson, Glasgow, N. B., Manufacturer; for a method of making chains of wire. Mar. 7.—July 7.

26. (45.) Charles Schafhautl, Sheffield, *York.*, Gent.; for improved gear for obtaining a continuous rotary action. Mar. 8.—July 8.

27. (30.) Charles Schafhautl, Sheffield, *York.*, Gent.; for an improved steam-generator. Mar. 8.—July 8.

28. (37.) John Barsham, Stepney Causeway, *Middx.*, Oxalic acid manufacturer; for improvements in the manufacture of oxalic and salacetecella. Mar. 8.—July 8.

29. (39.) Clinton Gray Gilroy, Argyle-st., New Road, St. Pancras, *Middx.*, Engineer; for certain improvements in machinery for weaving plain and figured fabrics. Mar. 15.—July 15.

30. (4.) Francis Brewin, Kent Road, *Surr.*, Esq.; for a certain new and improved process of tanning. Mar. 18.—July 18.

31. (47.) James Morison, Paisley, N.B., Manufacturer; for improvements on the jacquard machine, and on what is called the ten box lay, and on the reading and stamping machines used in making shawls and other figured work. Mar. 18.—July 18.

METEOROLOGICAL JOURNAL FOR MARCH, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month	Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer		Daily Temp	Solar Var.	Rad.	Clouds.		Wind.		Direction of wind		Lunation.	WEATHER, &c.
					Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Tuesday, 1	29·326	48°	29·155	48°	33°2	46°9	40°0	13·7	31°	10	9	2	4	S.S.E.	S.S.W.		Overcast; a violent shower of hail P.M., and a heavy gale of wind.
Wednes. 2	29·558	50	29·650	52	39·0	49·6	44·3	10·6	38	2	7	3	2	W.S.W.	S.S.W.	○	Fair; "*cirrus* and *cirro*-stratus."
Thurs. 3	29·750	52	29·835	53	40·1	49·9	45·0	9·8	38	10	8	1	1	W.	W.S.W.		Cloudy with light rain; fine evening.
Friday, 4	29·841	53	29·800	53	37·9	49·5	43·7	11·6	35	7	5	1	1	S.	S.W.		Fine evening; lowering; wind and rain at night.
Satur. 5	29·604	52	29·521	54	38·2	50·6	44·4	12·4	36	5	8	2	3	S.S.W.	S.W.		Squally; a storm of hail, wind, and rain at 5 P.M.; fine night.
SUN. 6	29·435	53	29·305	54	34·1	49·0	41·5	14·9	30	9	8	2	2	S.S.E.	S.		Rainy; fine night.
Mon. 7	29·540	53	29·528	55	32·0	50·8	41·4	18·8	29	3	7	1	0	S.E.S.	S. b W.		Fine with *cumuli.*
Tues. 8	29·502	53	29·536	54	33·9	39·6	36·8	5·7	30	10	10	1	1	N.N.W.	N.N.W.		Rainy; overcast throughout the day.
Wed. 9	29·600	51	29·446	52	30·1	47·5	38·8	17·4	29	1	9	1	2	S.W.	S.S.E.		Hoar frost; *cirro-stratus;* afternoon and eve. rainy.
Thurs. 10	29·508	52	29·484	53	37·6	51·0	44·3	13·4	35	9	10	2	2	S.W.	S.W.	☾	Cloudy; rainy evening.
Friday, 11	29·241	53	29·275	54	40·2	51·4	45·8	11·2	39	5	5	3	3	S.E.	S.E. b E.		Squalls of wind and rain; high wind at night.
Satur. 12	29·400	53	29·459	54	40·0	51·3	45.7	11·3	38	6	6	3·4	3	S.W.	W.S.W.		Do. do. do.
SUN. 13	29·715	53	29·770	55	38·7	30·6	44.6	11·9	37	2	3	2	2·4	S.W.	W.S.W.		Fine; stormy indications again at night.
Mon. 14	29·351	54	29·400	55	43·0	50·0	46·5	7·0	40	10	10	3	2	S.W.	W.S.W.		Tempestuous A.M.; rain till 4 P.M.; fine evening.
Tues. 15	29·200	54	29·459	54	37·8	50·1	43·9	12·3	38	4	4	4	3	W.	W. b N.		Strong wind, very high.
Wed. 16	29·875	52	29·982	53	34·2	45·6	39·9	11·4	32	6	5	2	2	W.S.W.	W.		Large *cumuli;* air drier; clear evening.
Thurs. 17	30·100	54	30·100	54	35·2	51·5	43·4	16·3	34	10	10	4	4	W.S.W.	W.S.W.	●	Cloudy, with high winds and *scud.*
Friday, 18	30·415	55	30·471	58	47·1	61·4	54·3	14·3	45	10	7	1	1	W.	W.S.		Fine warm weather; *cirro-cumuli* and *cumuli.*
Satur. 19	30·394	58	30·276	61	43·6	63·9	53·8	20·3	38	0	0	1	1	E.	S.		*Stratus* A.M.; fine clear weather, and very warm.
SUN. 20	30·302	60	30·303	64	41·9	65·4	53·6	23·5	37	0	1	1	1	W.	W.		Perfectly fine and serene; *stratus* at night.
Mon. 21	30·250	60	30·142	60	44·6	56·5	50·6	11·9	43	10	10	1	1	S.W.	S.W.		*Stratus* with mist; cloudy throughout.
Tues. 22	30·050	58	30·000	58	45·9	52·9	49·4	7·0	43	10	10	1	1	S.W.	S.W.N.		Small rain; overcast.
Wed. 23	29·884	57	29·721	57	35·3	52·5	43·9	17·2	34	9	10	1	2	S.	S.S.W.		Cloudy and lowering with small rain.
Thurs. 24	29·672	55	29·670	56	38·2	50·0	44·1	11·8	36	5	5	3	3	W.	W.		Fine A.M.; hail at noon; stormy indications at night.
Friday, 25	29·100	55	29·152	55	37·6	50·9	44·2	13·3	36	10	4	2·4	3	S. b W.	N.W.	☽	Rain till noon; wind very high.
Satur. 26	29·369	52	29·488	52	34·3	47·0	40·4	12·7	32	8	8	4	3	W.	W. b N.		Stormy, with *nimbi* discharging rain, sleet, and snow.
SUN. 27	29·700	51	29·627	52	28·0	50·1	39·1	22·1	25	1	6	1	2	S.W.	S.		Fine; *cirrus;* overcast at 11 P.M.
Mon. 28	28·835	52	28·816	53	35·9	46·8	41·4	10·9	35	10	10	2	3	E.S.E.	N.E.		Rain; *Cirro-stratus* and *scud;* wind high at night.
Tues. 29	29·685	51	29·750	52	34·0	48·0	41·0	14·0	33	5	5	3	2	W.S.W.	W.		Squally; evening fine.
Wed. 30	29·840	52	29·485	53	40·2	53·2	46·7	13·0	36	10	8	3	4	S.S.W.	W.S.W.		Rain and wind.
Thurs. 31	29·806	54	29·950	55	42·1	50·0	46·0	7·9	40	3	5	4	4	W.	W. b N.		High winds with *nimbi.*
Mean	29·673	54	29·665	55	37·86	51·09	44·47	13·23									

Bar. Max. 30·500 on the 18th.
Bar. Min. 28·760 28th.

Mean height at 9 A.M. 29·673
Mean do. at 3 P.M. 29·665

Ther. Max. 65°4 on the 20th.
Ther. Min. 28°0 27th.

Mean Tem. 44°47.

Lowest point of Rad. 25°, on the 27th.
Solar var. 13°23. Rain fallen 3·13.

THE

MAGAZINE OF POPULAR SCIENCE,

AND

JOURNAL OF THE USEFUL ARTS.

ON THE

GENERAL PRINCIPLES ON WHICH THE CLASSIFICATION OF NATURAL OBJECTS IS FOUNDED.

THE necessity for a classification of a multitude of any objects, as a preliminary to the study of them, is too obvious to require insisting on; it might be even assumed that no knowledge could be obtained without this previous arrangement, since that knowledge is primarily grounded on a series of comparisons, made by bringing those objects before our minds into juxtaposition, which possess certain analogies of form, structure, or qualities.

The facility of making a systematic arrangement depends on the precision with which each group of objects admits of definition, or on its possessing few and unvarying characters, which do not pass into one another by insensible gradations; thus, for the purpose of illustration, if we desired to classify the forms of space or extension, which are made the objects of geometrical reasoning, our arrangement would be perfect and immutable, for *form* being the sole character essential to the existence of each group, variations in that form, admitting of the most rigid definition, must constitute its basis. But when the characters distinguishing the objects are numerous, and not clearly definable, as is the case with all those of the material world, it is only by repeated tentative arrangements that we can arrive at one which will fulfil the requisite conditions.

There appear to be two principal purposes to which classification is applicable;—First, to furnish an arrangement which may enable a learner to ascertain the *name* under which a species, new to him, is described, and to serve as an aid to his memory in recalling the information he subsequently obtains; and, Secondly, to present a general and comprehensive view of that portion of creation to which the arrangement is applied in all its various relations. We shall confine ourselves, however, to the principles as applicable to the organic world only; and if we can succeed in elucidating the subject in this, its more complicated bearing, the application of those principles, on other occasions, will be readily conceived.

To understand the difficulties which must be overcome in our endeavour to attain either or both of these ends, we must constantly bear in mind that Nature affords us no assistance towards any arrangement beyond that into *species*, with which her classifications begin and end. *Species* are the aggregates of those individuals which possess perfect identity in all the details of their organization and of the faculties derived from it; and we are furnished with an unerring test of *nearly* universal application, by means of which we can ascertain the limits of species; namely, the power of giving birth to new individuals which always belong to that of the parent*. The number of species already known, living and extinct, is so great that they require to be collected into groups for the purpose of reference; which groups must again be combined into others more comprehensive. But though this synthetical mode of proceeding must always be that by which classifications are arrived at, yet the subject may be better elucidated by reversing the proceeding, by analyzing the most comprehensive sections thus obtained till we descend again, as it were, to the species.

A broad and obvious foundation for these primary divisions, is afforded to us by those striking characters of form and organization immediately connected with the mode of existence of the several species comprised in them: *Quadrupeds*, *Birds*, *Fishes*, and *Insects*, were admitted as obvious and natural divisions of animals from the earliest times, while *Trees*, *Shrubs*, and *Herbs* were recognised as corresponding ones under which the vegetable kingdom might be distributed; and as long as our acquaintance with organic creation was limited to that of but few species, comparatively speaking, and those but little understood, these vague divisions were found sufficient. But in proportion as the species which became known increased in number, and the laws of their organization were better ascertained, these divisions were found to be inadequate, and it has been gradually discovered that there exists no precise line of separation between any groups of species, however large or small they may be made, but that they pass insensibly into one another at some one of the numerous points at which they are connected.

Thus the division of *Quadrupeds* was extended, to receive the *Cetaceæ*, animals possessing the external form and the habits of fishes, but having the entire organization of terrestrial quadrupeds; and since they are deficient in the outward limbs, so generally characterizing the old division, the name of the class was exchanged for the more accurate one of *Mammalia*. Again, there was a large class of animals which, from possessing four external limbs, might have been ranked accordingly, but they were found to possess other, more important, characters, essentially opposed to those of quadrupeds in general,—the class in question being *oviparous;* with these were associated in time others which were organized in a similar manner, but varied greatly in external form; and a new division was instituted under the title of *Reptiles*, comprising *Tortoises*, *Lizards*, *Frogs*, and *Serpents*.

This class, the limits of which are, to this day, extremely vague and fluctuating, is especially adapted to illustrate the fact we have been alluding to, of the insensible gradation, by intermediate species, of one into another, with which it might at first appear to have little affinity. While the reptiles are characterized, in common with Mammalia, Birds, and Fishes, by an internal skeleton and vertebral column, by a double nervous system, pulmonary organs for breathing, and coloured blood, they are distinguished from the former by being oviparous,—from birds, by being terrestrial, and unprovided with organs for raising and propelling themselves in the atmosphere; while their being capable of existing for a length of time in water, their more torpid circulation, and simpler organization, indicates an affinity between them and fishes, which, in some respects, is so great, that the line of separation between these two, otherwise dissimilar, classes, is becoming daily less distinct. The *Polypterus bichir*, though ranked among fishes, differs from all others in the structure of its dorsal fin, and partakes of so many of the peculiarities of the order Serpent, that M. Geoffroy, who has described it at length, hesitates in which class to assign it a place; and there exist in the class of reptiles, anomalies yet more remarkable, which it will be worth our while to notice in illustration of our subject. The organs of respiration have ever been considered one of the most important characters on which systematic arrangements might be established; those animals which pass their life in water being provided with a modification of these organs termed *branchiæ*, by which they are enabled to abstract the air from the fluid that surrounds them, while those which are properly *terrestrial*, breathe by means of lungs adapted to bring the atmospheric air in immediate contact with the blood; but though there are many animals which can frequent, indifferently, both the land and the water, yet they, *generally* speaking, have only a pulmonary system, and when in the water, they are obliged to come to the surface to breathe; such are the crocodile, alligator, seal, walrus, water-rat, &c., and all the Cetaceæ. Fishes, on the contrary, having a branchial apparatus, cannot exist out of their element, because that form of respiratory organ is not calculated to receive the direct agency of the atmosphere. Now the *Batrachian* order of reptiles presents this remarkable phenomenon, that at different stages of their existence they have the two kinds of respiratory organs; at the early part of their life they are entirely aquatic animals, and breathe by means of gills, like fishes; while they subsequently undergo an outward metamorphosis commensurate with the important one which takes place in their internal organization, they acquire the lungs of terrestrial animals, and simultaneously acquire those external organs of locomotion which were unnecessary to them during their former mode of life: again, in the axolotls, sirens, and protei, genera of the same order, inhabiting lakes in Mexico and the Old World, we absolutely find the two modifications of respiratory organs coexistent in the same animal, which is thus truly *amphibious*, or can exist indifferently on the land or *in* the water. These few examples, which we might multiply indefinitely from all sections of the animal kingdom, will be sufficient to prepare the reader to understand the difficulties attending all attempts at classification, as we are now going to explain them.

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* The production of *hybrids*, or the offspring of individuals belonging to different species, is so limited, and the subject is so little understood, that, in a popular essay, it may be overlooked, especially as every increase in our knowledge of the laws of physiology tends to explain those apparent anomalies to the general principle, and to reduce the exceptions within the rule.

When we attempt to classify the species with which we are acquainted, we must place them in some succession, that is, our arrangement must be a *linear* one; and it is essential that it should be so, in order to accomplish one of the purposes we have stated to belong to such systems, that of making *it serve as a general index*, by which a learner may *refer to the great volume of* creation; to employ an analogy to which we shall again recur. But it is clear, from what we have already shown of the varied affinities of species, that while we place together in any *linear* arrangement, in whatever mode they may be grouped into genera, orders, and classes, those species which agree in one or more particulars of organization, habits, &c., we must inevitably separate them from others with which they are also allied, but which must be sought for in some remote part of our arrangement. The two objects of all classifications were, therefore, found to be incompatible together, consistently with the improvement of either. The greatest merit an *index* can have is that of easy reference; for the same reason that an index of words or *names* is best, as such, when they follow each other alphabetically, without sense or connexion; so to effect a classification of natural objects, which might answer an analogous purpose, it is only necessary to select some one obvious character, common to all the beings, to be arranged in each class, and the subdivisions of that class must be founded on distinct modifications of that character. Thus, for example, if the dentition be made the basis of subdivision of the class Mammalia, as was done by Linnæus, the presence of two canine teeth in each jaw will characterize the *order Feræ*, the absence of canine teeth that of *Glires*, the want of upper incisors that of *Pecora*, and so on. The class Fishes was subdivided by the same naturalist according to the presence, absence, or relative situation of the ventral-fins. Classifications formed on these principles are termed *artificial*, as being opposed to that of complicated affinities, by which Nature might be imagined to arrange a system.

It is found that there always exists a consistent relation between the physical organization of a being and its mode of existence; that the former has, in all cases, immediate reference to the great functions of life, and that if these functions are exercised in a similar manner by different species, there exists a general conformity to a common type in the physiological structure of those species. Thus, for instance, setting aside the more palpable agreement in form among animals that either move and dwell on the surface of the earth, or that can raise themselves in the atmosphere, or live in the water, an agreement which, as we have remarked, constitutes the basis for the chief divisions, it is observed that the larger animals, which derive their nutriment from the flesh of others, agree in their general features of structure and of habits, that their organs of digestion are in accordance with their food, and with the average degree of regularity in its supply. An animal capable of subduing most others by its superior strength and agility, must inspire a terror among those with which it is stationed, that must augment the difficulty of procuring its food; such a creature has to endure frequent privation, and its organization is, consequently, modified to suit this new condition in its mode of existence. Those animals, on the contrary, which are not carnivorous, and have their food more constantly in their power, not being subjected to occasional abstinence, do not require the structure of the digestive organs which the former do; but, on the other hand, must be enabled to collect, digest, and assimilate the cruder vegetable substances on which they feed. The ox, the antelope, the horse, the hare, the elephant, and the giraffe, are all equally herbivorous; let us trace a few of the modifications of their mode of life, which are connected with such a difference of form and habits. The first not being adapted by its form for rapid motion, which might enable it to escape from its carnivorous enemies, is provided with weapons of defence and great muscular power; the structure of the second, expressly contrived for great velocity, was incompatible with strength; and though the tribe is furnished with horns, yet they are generally inefficient as weapons against the beast of prey, from which the antelope escapes by its superior agility. Why the absence of horns, in the third tribe, is connected with the undivided hoof and the simpler stomach, is one of those mysterious problems in organic creation which we can never solve; but the *fact* being ascertained, it is clear we must conform our arrangements of these tribes to this general law: the trunk of the elephant, the extensible tongue of the giraffe, the unwieldy bulk of the former, and the long neck and peculiar form of the latter are all referrible to varied organization and habits attendant on it.

Now, if we were to endeavour to arrange the species belonging to these few tribes of animals, by some one obvious and sensible character, so that a learner, on meeting with an individual new to him, might at once, by means of this system, assign it its place, and discover its congeners, which character must we take? We should find that no one character would enable us to form a rational arrangement, but that we must have recourse to several, if we wish to avoid the most glaring inconsistencies, and to *all*, if we aim at a satisfactory one.

But since our arrangement must, after all, be a linear or tabular one; that is, the species must follow each other in their respective divisions and classes, instead of being presented to the eye, or to the mind, like a map, as it were, of the species woven together according to their complicated affinities; and since we cannot construct such a map so as also to serve as a classification, we must be so far artificial in our arrangement as to adopt a tabular form of this kind:—

Division	Class	Subclass	Order	Genus	Species	Variety
		Subclass	Order	Genus	Species	Variety
	Class	Subclass	Order	Genus	Species	Variety.
		Subclass	Order	Genus	Species	Variety.

To render this table as consistent as possible with the spirit of a natural arrangement, or so that the species may be grouped near to one another, according to their alliances, we must previously determine the relative importance of the various functions, and of the organs by which they are exercised, and then make the series we obtain by this proceeding the basis of our subdivisions; since we may reasonably expect that the more important function would require a more constant conformity to some type of the organs pertaining to that function, and that, by making this type the characteristic of the more comprehensive subdivision, we should bring the greater number of species under it.

But with our extremely limited knowledge of the functions of organs, we have little to guide us in forming such an estimate, and our inquiries are *constantly baffled* by finding new deviations from that ideal *type we had formed from* repeated observations of numerous species, as representing the structure most characteristic of the class or any of its subdivisions: with each discovery of such a deviation, or of some hitherto unknown organ, or of some mode of action not previously referred to an organ, the first impulse, in our anxiety to perfect the arrangement, would be to separate the species thus distinguished from those with which it was previously associated, and to erect it into a new genus with a new abstract of characters, including the peculiarity recently dicovered. If it were afterwards ascertained that a similar modification existed in some other species, hitherto supposed to have little or no affinity with the former, it would be a presumptive reason for suspecting that we had either been mistaken in our opinion as to the degree of that affinity, or else that the peculiarity in question was of minor importance, and derived from some cause not necessarily connected with the great functions of life.

This dilemma can only be resolved by a careful investigation of the species in question, and by a diligent search after the same modification in others; and, if found, it must be ascertained whether its presence throws any new light on their structure: till all these questions are answered, different naturalists will admit or reject the formation of a new genus to contain the species first alluded to, according to their view of the relative importance of the peculiarity in question; there is always a reluctance to form a new genus on slight grounds, because the system of classification is thereby rendered more and more complicated. Hence it appears that the *tendency* of what is termed a natural arrangement of species, is to resolve the more comprehensive sections into the separate species which constitute them, till each stands alone by its aggregate of characters, and yet connected by newly-discovered affinities with an increasing number of others; and since but few of these affinities can be displayed by a tabular arrangement, the very essence of classification is thus destroyed in the endeavour to improve it.

But even admitting this *tendency* of natural systems of arrangement to lose the character of a classification, and to become useless as an index of reference, or as an aid to the memory, yet their importance in a scientific point of view far more than counterbalances this defect; we will elucidate the comparative advantages of a natural over an artificial arrangement by means of the analogy before suggested. Suppose we were presented with a voluminous work, from which we expected to derive instruction and amusement, and suppose we were provided by some previous readers, both with an *alphabetical index* of the words and names contained in the volume, and with a well-digested, though imperfect, *analytical table of contents*, from which of these two should we gain the most knowledge of the nature of the book and of its contents? It is true that by the index we might find any *name* which occurred in the text, and be referred to the page where it would be found in its proper relation to the rest of the matter; but from the index alone we learn nothing but that the name begins with a certain letter, and is placed in consequence next to another having the same initiatory consonant, but with which it may have no other connexion whatever, and yet it could *not be separated* from that other word without destroying the value of *the index* as such. On the contrary, if we study the table of contents, we obtain a general insight into the train of reasoning pursued by the author, and of the logical arrangement of his work, and we are thereby better prepared to read and understand it; for in every case it is from the body of the work only that we can derive true knowledge; and all that either an index or table of contents can avail us is to point out to us the best way of pursuing our studies, and to show us the connexion of what we have acquired with what we have yet to attain.

It should be here observed, that the application of arrangements strictly artificial, to the animal kingdom, has long been given up, for reasons, too, which clearly prove the justice of the objections we have urged against the principle of such systems. The very complexity of animal organization, while it invites more constant and reiterated investigation, enables us, by a comparison of its component parts, to arrive at more conclusive inferences respecting their functions and their relative importance; and each new examination confirms the truth of what we have already stated, as to the constancy, *within certain limits*, with which forms of structure are associated with certain functions and habits: hence, the primary divisions of every classification of animals remain nearly constant; and it is only the subordinate ones of *orders* and *genera* which fluctuate. Thus, for example, when an animal was found which had all the essential characters of the genus *Felis*, but did not possess retractility of its claws, naturalists, who regarded this as a character pertaining to animals intended to spring on their prey, might agree as to the nesssity for forming a new genus, and would discover a new, and, hitherto unsuspected, relation between genera of the same order, *Carnivora;* but we are so convinced of the harmony existing in the plan of creation, as to feel certain that we shall never meet with an animal which will combine the characters of a carnivorous and of a ruminating one, or having the complicated digestive organs of the latter, with canine teeth and claws. Again, among the species composing the sub-order of plantigrade carnivora, there occurs one, the Kinkajou, which has a prehensile tail, and a tongue capable of considerable protrusion, like that of the Manis, thus presenting an affinity between a carnivorous species and genera of the widely different orders of Quadrumana and Edentata. If we descend to the invertebrated division, we find the limits of even classes fluctuating: thus, there are many species of which naturalists are not yet agreed, whether to refer them to Articulata or Mollusca; but all these doubtful cases bear too small a proportion to the more definite ones, to cause any uncertainty as to the permanence of such general divisions as have for a long time been adopted in the animal kingdom.

The case, however, is widely different as regards the vegetable kingdom, the simplicity of the organization, and our uncertainty as to the functions exercised by the component parts, an uncertainty chiefly arising from the difficulty of making observations, prevent our yet deciding on even the primary divisions, and still more on fixing their limits with anything like distinctness; the numerous species might therefore seem still to justify the use of an artificial arrangement; and that of Linnæus being unquestionably the best ever contrived, and possessing merits of the highest order, it will probably, for some time to come, be employed even by many who acknowledge the imperfect principle of all artificial systems.

It is necessary, however, to put beginners, who have not considered the subject, on their guard, lest they should, as is but too commonly the case, mistake a facility in applying this system, for the acquisition of real knowledge of the science of Botany: we shall therefore proceed to show that the Linnæan system can only lead to an acquaintance with the *names* of plants, and not to that of their mutual affinities, which, as we have stated, is the only true foundation of real knowledge.

Most of our readers are aware that, besides the two primary divisions of species into flowering and flowerless plants, the former are arranged under two classes, distinguished by peculiarity of structure, which, from their generality and constancy, must be important; though, in the present imperfect state of our knowledge of the subject, we are even unable to indicate at what period the elementary organs, which are apparently the same in all plants, undergo that change in their functions that leads to such remarkable results. All that we know is, that, *generally*, certain species which proceed from seeds with only one, or with alternate cotyledons, have the structure of their stem entirely different from that which is perceptible in the stems of plants proceeding from seeds with two, or with verticillate, cotyledons: that the leaves also of monocotyledonous plants differ from the reticulated texture of those of the other class, in having their veins parallel, and that their flowers are characterized by a *ternary* instead of the *quinary* division of their component parts, which is usually met with in those of the dicotyledonous class.

It is impossible to say whether we shall ever be able to explain the origin of the numerous and singular exceptions to these general characters of the two classes; but it is clear that it is by endeavouring to do so that we shall make the greatest progress in ascertaining the laws of vegetable physiology, and our investigations will be mainly assisted by every improvement in correct classification of known species, by which they may be grouped according to their most numerous natural affinities: it is also impossible for any one to make a progress in the knowledge of that physiology, unless he is habituated to keep these affinities constantly before his mind during his researches.

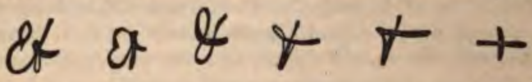
The Linnæan system is strictly an *artificial* one, and was intended to be so by its great author: it is founded on the selection of modifications of one organ only as the basis of the divisions; we might therefore anticipate, by the analogy of what we have shown to be the consequence of such a principle in the classification of the animal kingdom, that the most glaring violations of natural affinities would be the result. It is found by observation that the precise *number* of any of the parts of the reproductive organs of plants is liable to constant derangement, from the laws which govern their developement; that is to say, that of several plants having decidedly the closest connexion in every peculiarity of structure, qualities, and even form, one will have only two stamens, while another has four, and a third, perhaps, five or ten. All these would be separated and placed in different classes in the Linnæan system; and since the primary distinction of structure of the seed, and all its attendant consequences, formed no part of the plan on which that system was built, the student who employs it, and who is not aware of its true principle, is habituated to associate together in his mind monocotyledonous and dicotyledonous plants, provided they have an equal number of stamens or styles.

It would be foreign to the object of this essay, to enter here into any account of the principles on which the normal number of the parts of fructification varies in plants; but we will show, by one or two examples, the inconsistencies resulting from the artificial arrangement in question. The establishment of the very first class is a fallacy of the most fatal tendency; the existence of one stamen only, though constant in certain species, is the result of that peculiar law of non-developement of some of the parts arranged round a central axis, when, from their position, they are crowded together: the botanist who is imbued with the spirit of natural affinities, regards those species thus distinguished only as regular and constant deviations from the type of the family to which their other characters unite them; a deviation he can explain by the principle just alluded to. The Linnæan student, who always commences his studies with mastering the system, on account of the aid it affords to him in his pursuits, becomes habituated to the idea, that the character of having only one stamen is just as important as that of having three, six, five, or ten: when he sees a specimen of the genus *Maranta*, he does not, as he ought to do, view it as one of a family, having a ternary arrangement of its flowers, but that two of its anthers ever remain undeveloped, or *abortive*, as botanists term this state of non-appearance of any organ which exists in the regular or ideal type of the order. That this is the true explanation of the deficient number of stamens, is shown by the existence of the petaloid filaments in the plants in question. Of all the natural orders of plants, of those which are distinguished by the greatest number and constancy of characters in the species composing it, those of palms and grasses are pre-eminent for their distinctness; now of the first of these two we find the genera in the Linnæan system distributed under four different *classes*, and eight different *orders* or subdivisions of those classes. The same unnatural separation of genera takes place with respect to the grasses; and in both cases it is caused by deviations either in the number of stamens, or by the equally explicable anomaly of the male and female flowers being distinct on the same or on different individuals.

But this is not the whole of the incongruity resulting from this application of one character only to the distribution of species of the vegetable kingdom, not only are those separated which are really closely allied, but others are brought into juxta-position which have no relation whatever to each other; in the instances to which we have been alluding, genera of grasses and palms are placed together with dicotyledonous ones of the most opposite qualities and characters. It must not be supposed that these are singular instances collected to justify a partial statement; there is not a class or order throughout the system which does not exhibit nearly equally striking absurdities, as for example, the separating a few genera of the well-defined order of *Leguminosæ* from the rest, because *the union of the filaments* is more perfect, or because *it does not exist at all*, as in the *mimosæ*.

and Arabian algebraists, as any one may see by looking into the works of Colebrooke, Taylor, and Strachey, the translation of Mahomed ben Musa, by Dr. Rosen, and many others. Contractions, however, are of early date in literary composition; and very naturally suggested others, of words and phrases which were of such perpetual occurrence as those employed in any particular and defined course of research: hence the symbols to designate the planets (and the metals which were supposed to partake of the influence of those planets severally), the letters of the alphabet to designate the particular kind of proposition under consideration in logic, and a hundred others which might be readily quoted, but of which we shall only instance the employment of letters of reference to designate points, lines, and spaces, in geometrical figures, and to designate the unknown quantity in algebra, and ultimately (to give generality to a result), to designate also the known or given quantities employed in investigations in the same science. A brief account of some of these, from the most authentic sources, where direct sources of information exist on the most probable conjectures that we have seen, or which may have occurred to ourselves, where no direct authority exists, will be occasionally given in this Journal. We commence with a case of the latter kind, solely because the signs in question are, except the symbols of number, the earliest that occur to the calculator.

1. The sign + (*plus*), indicating addition, was early expressed *et* (and); the forms of its gradual contraction from the *manuscript* form (a good deal similar to the early printed forms) will be apparent from the annexed series of transformations, all of which are easily verified by a



reference to existing documents. Various contortions of the first symbol of this series may be found in early books and MSS.; but in the one case they are merely for ornamental printing, and in the second for ornamental writing. The spirit which dictated them still maintains its ground in all parts of the civilized world.

2. Every one knows, that even in printed books, it was the *general custom* to omit several of the vowels, and draw a line above the preceding letter, to indicate that the vowel should be read there, or as forming an integral part of the sound of which the marked consonant was the commencement. The same was also done for the *m* and *n*. The word *minus* (less) was therefore thus written, $\bar{\text{m}}$ $\bar{\text{n}}$ s. Brevity and rapidity led to the substitution of the mere line for the word, and hence is derived the – itself.

A POPULAR COURSE OF CHEMISTRY.

III.

Elements.—Nomenclature.—Definite Proportions.

"There are Four Elements, Fire, Air, Earth, and Water."—This aphorism is found in almost every work on the rudiments of natural knowledge; it is early instilled into the mind of the youthful student, and never forgotten by him at any future period of his life.

In the earlier times, every philosopher appears to have taken his own imagination for his guide; Aristotle and Empedocles, accordingly, framed the doctrine of the four elements, asserting that they possessed *absolute simplicity*, and that all other bodies were formed by their combinations. This doctrine, supported by such high authority, was well calculated to seduce the mind; it was accordingly very generally received by all polemical schools for a long succession of ages; it was no less eagerly embraced by the ancient poets; the moderns closely followed their example; and gradually, from the classic pages of both, the four elements have found their way into ordinary writing and discourse, until at the present day they are "familiar in our mouths as household words." The alchymists were the first who had the temerity to oppose this doctrine of Aristotle; for temerity it must be called, when we reflect that his name operated like a charm, and held the minds of men in such a state of intellectual bondage, that statutes were actually framed in some universities, which required teachers "to promise on oath, that, in their public lectures on philosophy, they would follow no other guide!" The alchymists, nothing daunted by the fulminations of the polemical schools, boldly put forth a new doctrine of *Three Elements*, viz. Salt, Sulphur, and Mercury, most unequivocally stating that all other bodies were produced from these, and not from the four common or vulgar elements. This new doctrine was promulgated to a most singular and remarkable extent. Paracelsus particularly distinguishes himself as its champion, invoking *sideric* influence to his aid; however ridiculous this may now appear, yet his enthusiastic ravings certainly aroused mankind from their lethargy, and shook the ancient doctrines to their very centre.

The ancient elements and the alchymical elements both found their strenuous advocates; those who were "content servilely to follow authority, without indulging in a liberty of contradicting," defended the ancients; whilst the cause of the alchymists was espoused by such as were "followers of novelty and attached to things more curious than useful." Neither advocates, however, were "duly prepared and fitted to the business of the interpretation" of Nature. Her secrets were not to be attained by indulging in subtle metaphysical disputations, or by a few phantastical operations of the furnace.

Inductive philosophy at length arose, and, by the searching light of experiment, soon demonstrated the true nature of both classes of elements, and assigned them proper stations in the science of chemistry.

This *science of experiment* does not recognise the four elements in *their ancient acceptation;* it does, however, recognise two of the three elements, viz., *sulphur* and *mercury*, but only so far as they accord with the following definition:—Elements are bodies, which the utmost skill and power of the chemist cannot resolve into any other forms of matter; it is not to be supposed from this, that they possess *absolute simplicity*, or are incapable of being ultimately decomposed, far from it; but until such decomposition is effected, they must be considered as *elementary*, or *simple substances*. Now, *fire*, *air*, *earth*, and *water*, have all been decomposed or resolved into new and simpler forms of matter; so has *salt;* but *sulphur* and *mercury* as yet resist the most refined chemical torture, and will confess nothing whatever respecting their hidden constituents.

The number of substances which the chemist is reluctantly compelled to acknowledge as elementary is very great, namely, *fifty-two*. They are here subjoined, and the decomposition of any one of them would be a most transcendent discovery.

Elementary or Simple Substances[*].

I. *Supporters of Combustion.*—Oxygen, 8; Chlorine, 36; Iodine, 125; Bromine, 78; Fluorine, 18?

II. *Inflammable Substances.*—Hydrogen, 1; Nitrogen, 14; Sulphur, 16; Phosphorus, 12; Selenium, 40; Carbon, 6; Boron, 20?

III. *Metals.*—Aluminum, 10; Antimony, 65; Arsenic, 38; Barium, 70; Bismuth, 72; Cadmium, 56; Calcium, 20; Cerium, 48; Chromium, 28; Cobalt, 30; Columbium, 185; Copper, 64; Glucinum, 18; Gold, 200; Iridium, 96; Iron, 28; Lead, 104; Lithium, 10; Magnesium, 12; Manganese, 28; Mercury, 200; Molybdenum, 48; Nickel, 28; Osmium, 100; Palladium, 54; Platinum, 96; Potassium, 40; Rhodium, 45; Silicium, 8; Silver, 110; Sodium, 24; Strontium, 44; Tin, 58; Titanium, 24; Tungsten, 100; Uranium, 217; Vanadium, 68; Yttrium, 32; Zinc, 32; Zirconium, 30.

These elements are all *ponderable* bodies. They can be all confined, weighed, and measured: they present every variety of mechanical properties,—solid, liquid, aëriform, or *gaseous;* this term being employed to denote such elastic fluids as differ in their chemical habitudes from the atmosphere, which is exclusively called *air*. The term *gas* was first introduced into chemistry by Van Helmont; and, at the present day, it is applied, strictly speaking, to such aëriform matters as are *permanently elastic* under all pressures and temperatures; the term *vapour* is applied to aëriform matters which, upon pressure and cold, *condense* into the liquid or solid state. *Oxygen* is an invisible, permanently elastic *gas*, without taste or smell, and may be breathed for a short time without danger. *Chlorine* is a bright-yellow *vapour*, of suffocating and poisonous odour, condensing into a bright yellow liquid by cold and pressure.

Iodine is a dense steel-gray crystalline *solid;* when heated forming a beautiful violet-coloured *vapour*, very irritating and poisonous, which condenses into brilliant crystals upon cooling.

Bromine is a deep reddish-brown liquid, of a disagreeable smell, and very deleterious to life; when heated forming a brownish-red *vapour;* when cooled congealing into a brittle solid of the same colour.

Fluorine has not hitherto been obtained in an insulated state; for its powers of combination are so great, that no body has been found capable of resisting its energetic action; it is the modern *alkahest*, or

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[*] The numbers here attached are explained at page 304.

universal solvent. None of these elements are found in an uncombined or native state: they all support combustion in some degree or other, and hence they are frequently styled *supporters of combustion;* they all unite with each other with the exception of fluorine.

Hydrogen and *Nitrogen* are both invisible permanently-elastic gases, without taste or smell, both fatal to animal life and flame; hydrogen is combustible, and the *lightest* substance known; nitrogen is incombustible, and fourteen times heavier than hydrogen, bulk for bulk; oxygen is sixteeen, chlorine thirty-six, and iodine vapour one hundred and twenty-five times heavier than hydrogen. This comparison of the relative weights of equal bulks of different bodies, is called *specific weight* or *specific gravity.*

Hydrogen, being the lightest substance known, is taken as the standard of unity amongst *gaseous* bodies and *vapours*, whilst *water*, by reason of its abundance, and the facility with which it may be obtained nearly pure, is assumed as the standard of unity for the specific gravity of *liquids* and *solids.*

Sulphur and *Phosphorus* are both *solids;* the one yellow and brittle, the other orange-coloured and tenacious; both are highly inflammable and volatile at a moderate heat, sulphur burning with a pale-blue flame, phosphorus with a splendid yellow flame; sulphur is not poisonous,—phosphorus is highly so; both are insoluble in water, the one nearly twice, the other only once and a half, as heavy as that fluid: the *vapour* of sulphur is sixteen times heavier than hydrogen, that of phosphorus twelve times. *Selenium* is a *solid* of a reddish-brown colour, and metallic lustre, soft and brittle, more than four times heavier than water, forming a dense yellow *vapour* by heat, which condenses by cold; the odour is very peculiar and poisonous.

Carbon and *Boron* are also *solids*, the one in its pure state, crystalline and brilliant, the *hardest* substance with which we are acquainted, namely diamond; the other a *soft* brown pulverulent substance; both are combustible, burning with much brilliancy, insoluble in water; the first being more than three times, the second only twice, as heavy as that fluid. Of these elements, only *two* are found in an uncombined or native state,—these are *sulphur* and *carbon;* they are all inflammable with the solitary exception of *nitrogen;* hence they are frequently called *simple inflammable substances:* they not only combine with each other, but with the *supporters of combustion*, and give rise to a numerous and most important class of compounds.

Now follow the *metals:* they are all *solid* at ordinary temperatures, with the exception of *mercury*, which, in these latitudes, is *fluid*, but it may be solidified by intense natural or artificial cold; the metals present us with various grades of aggregation, from hardness to softness, from tenacity to brittleness. They are all *opaque* with the exception of *gold* and *silver*, which if extended into thin leaves are then *transparent*; they have all peculiar brilliancy, though of various colours; this is known as *metallic lustre.* Another distinguishing feature is their very different relative weights, or specific gravities; thus *platinum* is upwards of twenty-one times heavier than water; it is, therefore, the heaviest substance known: *gold* is nineteen, *mercury* thirteen, silver ten, bismuth nine, *copper* eight, *iron* seven, and *tellurium* six times heavier than water; all these metals of course *sink* in it. *Potassium* and *sodium* are the *lightest* metals,—far lighter than tellurium, or even *water*, for they *float* upon it as buoyantly as cork.

Such are a few of the most obvious physical characters of the metals; and their chemical habitudes are no less remarkable; they are all, with one exception, capable of fusion by heat, though requiring very different temperatures for this purpose. Mercury is always fluid at common temperatures; potassium melts at a heat below that of boiling water; lead melts at the heat of boiling oil; gold, silver, and copper, melt at a bright red heat; iron and cobalt require a white heat for their perfect fusion; osmium, iridium, rhodium, and platinum, require the most intense heat for their fusion that the chemist can produce. Titanium has never yet been fairly melted.

Platinum, gold, and silver, undergo no further change when melted; they maintain their metallic lustre, and hence are often called *noble metals*. Iron, copper, lead, and tin, when melted or heated, quickly tarnish. Potassium and sodium tarnish upon mere exposure to air*.

The metals, generally speaking, are capable of combining with each other; they all unite with oxygen, chlorine, iodine, bromine, and fluorine; many unite with sulphur, phosphorus, selenium, and boron, some few with hydrogen. Several of these compounds are virulent poisons, others valuable medicines; many have important uses in the arts and manufactures, and none are wholly devoid of interest.

Iron is the only metal that combines with carbon, and there is not even a solitary instance of the combination of a metal with nitrogen.

Very few of this numerous class of elements are found in a native state; platinum, gold, silver, mercury, palladium, and rhodium, are probably the only ones which occur in such purity, as to merit the title of native metals. Independently of the direct union of the Elementary substances with each other to produce compounds, these compounds will unite with others, and form a new and extended series of products; to render this more intelligible; let us suppose oxygen uniting with copper, a compound is thus produced by the direct union of *two elements*. Again, let us suppose oxygen uniting with sulphur, another compound is in like manner produced; now, both these *compounds* are capable of uniting with each other to produce a third compound, so that a vast number of substances are thus presented to the chemist; and it is a matter of no small difficulty to arrange, classify, and confer appropriate names upon them all, as will be seen hereafter. Such, then, is an outline, and a very general one, of the properties and habitudes of the *fifty-two ponderable elements*; the manifold results of whose direct and indirect combinations present us with all the forms of matter with which we are acquainted, that is to say, as far as the present state of chemistry enables us to determine.

It is, however, highly probable that many of the *elements* are *compounds* of more highly attenuated forms of matter, closely allied in their nature to Light, Heat, or Electricity, which are called *imponderable elements*, "as they can neither be confined nor submitted to the usual modes of examination, and are only known in their states of motion as acting upon our senses," or upon the ponderable, and, consequently, grosser forms of matter.

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* An effect due to their union with oxygen.

However strong our suspicions may be of the compound nature of the ponderable elements, we must not indulge in any theoretical views or speculations concerning them, for chemistry is purely a science of experiment, and from experiment alone must we draw our conclusions; the torch of analysis will, doubtless, ere long throw light upon the hidden constitution of the elements.

Chemists are far from despairing to effect the decomposition of the elements, when they have so many triumphant proofs of the powers of analysis in unfolding mysterious combinations. For example, it was by the experimental examination of the ancient element *air*, that Dr. Priestley discovered it not to possess *absolute simplicity*, (as philosophers and disputants had for ages maintained,) but that it consisted of *two* gases; namely, *oxygen* and *nitrogen*, perfectly distinct and opposite bodies, *mixed* together in certain invariable proportions*.

Mr. Cavendish discovered that the ancient element, *water*, could be decomposed, and announced the wonderful and almost incredible fact of its being a *compound* of *two* gases, namely, *oxygen* and *hydrogen*.

Sir Humphry Davy experimentally demonstrated that the *earths* were all compounds of *oxygen* with *metallic* or *inflammable* matters; and that the *alkalies* which, like the earths, had always been considered as simple bodies, were likewise compounds of *oxygen* with metals of extraordinary habitudes, namely, *potassium* and *sodium*; continuing his splendid and unrivalled researches to the examination of *fire*, he proved that it was not an elementary form of matter, but the *result* of every intense chemical action.

But when these illustrious philosophers endeavoured to penetrate yet further, and to reduce the newly discovered bodies into other yet simpler forms of matter; they found them to resist and baffle the utmost exertions of analytical skill, and therefore they were compelled to acknowledge their utter ignorance of the nature of such bodies, and award unto them the title of Elementary or Simple Substances.

The above are magnificent examples of the triumph of Inductive Philosophy and experimental skill over scholastic dogmas and visionary speculations; with these before him, the chemist continues his researches in the ardent hope of decomposing some of the elementary bodies, and thus yet further enlarging the boundaries of natural knowledge.

In chemical language, therefore, we say, that the *four ancient elements* are *compounds* of simpler forms of matter, of whose constitution we know *nothing*; and although custom sanctions the mention of "the four elements, watery element, fiery element, conflicting elements, &c.," in common language; yet in the laboratory of the experimentalist, the title of *elements* is only conferred upon the bodies which have been enumerated in the foregoing pages.

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* It cannot be too early or too strongly impressed upon the mind of the juvenile student, that the ATMOSPHERE *is a mechanical mixture of* oxygen and nitrogen, and *not a chemical compound of* these gases.

NOMENCLATURE.

The *elements* may with much justice be called *the Alphabet of Chemistry*, which is very easily learned, but the *language* or *nomenclature*, formed by its varied combinations, is rather difficult of acquisition; as, however, it is impossible to study chemistry without the nomenclature, a slight sketch is here indispensable, and by furnishing it thus early, much future inconvenience will be avoided. We shall not have to digress from the immediate subject of inquiry, in order to explain technicalities, nor yet to enclose a hasty interpretation of them between parentheses, or to consign them altogether to notes at the foot of the page.

Oxygen, chlorine, iodine, bromine, and fluorine (?), are capable of entering into combination with each other, and also with all the other elementary bodies, producing two classes of compounds.

Those which are not acid are denoted by the termination *ide;* thus, *oxide* of chlorine, *chloride* of carbon, *iodide* of mercury, *bromide* of phosphorus, *fluoride* of calcium; and where more than one compound of this kind is produced, the terminations *ous* and *ic* are used to designate the relative proportions of the supporters of combustion. Thus nitrogen forms two oxides, that containing the *least* oxygen is called the *nitrous* oxide, that containing the *most* the *nitric* oxide. The acid compounds are similarly designated; thus, *nitrous* and *nitric* acid, *sulphurous* and *sulphuric* acid; and where there are intermediate compounds, the term *hypo* is occasionally added to the acid next above it in point of oxidizement, thus *hypo-nitrous acid* signifies an acid compound, intermediate between the nitrous and nitric acids. The various compounds of the metals with oxygen are usually best distinguished by prefixing to the word *oxide* the first syllable of the Greek ordinal numerals. Thus the *protoxide* of a metal denotes the compound containing the *smallest* quantity of oxygen, or the *first* oxide which the metal is capable of forming; *deutoxide* denotes the second oxide which the metal forms, *tritoxide* the third, and so on, until the metal is perfectly saturated with oxygen; the compound then, if not acid, is called *peroxide.* In like manner, protochlorides and perchlorides, protiodides and periodides, &c.

The acids terminating in *ous* produce compounds in which the termination *ite* is used: whilst those ending in *ic* form compounds in which the ending *ate* is used. Thus the combination of sulphurous acid and potash, is a *sulphite* of potash; that of sulphuric acid and potash, a *sulphate* of potash, &c. The combination of a metal with oxygen which is called its *oxidizement*, is an essential preliminary to its combinations with acids; no metal combines directly with an acid, but invariably in the state of *oxide*, this must be always remembered. Thus we say, sulphate of *iron*, nitrate of *copper*, &c., meaning sulphate of the *oxide* of iron, nitrate of the *oxide* of copper.

The terms *proximate* and *ultimate* elements may be here explained; thus the proximate constituents of sulphate of iron, are *sulphuric acid and oxide of iron*, because this substance results from the direct union of these two bodies; the *ultimate* elements of which, are sulphur, oxygen, and iron. When the same acid combines with more than one oxide of the same metal, the first syllable of the Greek ordinal numeral is in that case applied to the acid; thus the *proto-sulphate* and *per-sulphate* of iron, signify the combinations of sulphuric acid with the protoxide and peroxide of iron: it is more convenient than saying sulphate of the protoxide of iron, or sulphate of the peroxide of iron. The compounds of the elementary inflammable bodies with each other, and with the metals, are denoted by the termination *uret*, thus, *sulphuret* of phosphorus, *phosphuret* of carbon, *carburet* of iron. The terms *bi-sulphuret*, *bi-phosphuret*, &c., applied to compounds imply that they contain *twice* the quantity of sulphur, phosphorus, &c., existing in the first respective sulphuret and phosphuret*.

In addition to this abstract of chemical nomenclature, it is necessary to remark, that there are many bodies which have borne names for several ages, and these it would be not only pedantic, but inconvenient to alter. Thus ammonia, is a *hydroguret of nitrogen*, chemically speaking, but it is never so called, the term being very uncouth; potash, soda, magnesia, lime, baryta, strontia, silica, glucina, and yttria, are respectively *oxides* of potassium, sodium, magnesium, calcium, barium, strontium, silicium, glucinum, and yttrium, they are sometimes so called, but far more frequently by their old names, potash, soda, &c., &c., and their compounds respectively nitrates, nitrites, sulphates or sulphites of potash, soda, magnesia, lime, &c., &c. Thus, again, water retains its common appellation, although strictly speaking it is a *protoxide of hydrogen.*

Chemical nomenclature is not invented for the purpose of mystifying compounds, as was the case with alchymical language, but for the purpose of simplifying expressions and speaking with precision in terms which have reference to the source, nature, obvious property, or composition of a substance. Thus the term *oxygen* implies that it produces acids†; *chlorine*, that it has a greenish yellow-colour; *iodine*, a violet-colour; *bromine*, a suffocating and disagreeable smell; *fluorine*, is so named from the circumstance of its being yielded by fluor-spar. The term *hydrogen*, implies that it is a generator of water; *nitrogen*, of nitric acid; *carbon* is the Latin name for charcoal; *boron*, signifies that it is the basis of boracic acid. Then amongst the metals recently discovered, the same description of nomenclature is employed.

Thus, *aluminum*, signifies that it is the basis of alumina or clay; *barium*, that it exists in a ponderous mineral called heavy-spar; *cadmium*, that it is usually associated with other metals; *calcium* is so named, from being the metallic base of lime; *chromium*, from the variety of colours which its combinations assume; *columbium*, from being originally found in America; *glucinum* is the basis of the earth *glucina*, which has a sweet taste; *iridium*, produces compounds which are iridescent; the term *lithium*, implies that it is extracted from lapideous bodies; *magnesium* from the earth, originally found at Magnesia, in Spain;

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* Part of this abstract of nomenclature is copied from Mr. Brande's *Manual of Chemistry*, 3rd edition.

† This term is highly objectionable, for oxygen is equally active in producing *alkalies*, and bodies which are neither acid nor alkaline, viz., *oxides*; this single term oxygen, exerts a baneful *influence* over the nomenclature, and is apt to impress upon *the student that it alone is the* generator of acids, so the French chemists thought *when they conferred the name*, and now it is too late to alter it for any other.

osmium, implies the peculiar odour of its vapour; *potassium*, the basis of potash, which substance was so named in consequence of being found in the ashes of plants, when burned in pots or crucibles; *rhodium*, implies the red colour of its compounds; *silicium*, that it is the basis of the earth, silica or flint; *sodium*, the basis of soda, which substance derived its name from being originally found in the plant called *salsola soda; strontium* is the basis of the earth strontian, so called from the circumstance of its being originally found at Strontian, in Argyleshire, and *zirconium* is the basis of the earth, zirconia, originally found in Ceylon. Some of the metals are also named after the planetary bodies, but this is a relic of alchymy, which might as well be avoided in the present state of science; thus, *uranium* is so called in honour of Uranus, *palladium* of Pallas, *cerium* of Ceres, &c.

It was the fashion of the alchymists to call *gold*, *silver*, *iron*, &c., by planetary names, viz., the Sun, Moon, Mars, &c., to which they imagined them to possess some relation, or even by more fanciful appellations, as the king, the queen, the warrior, thus mystifying and destroying the original simplicity of their names as conferred upon them by the Hebrews, in whose language, the word *zeb*, denoting *gold*, signifies to be clear, to shine; *keseph*, the name of *silver*, refers to its pale colour; *berezel*, that of *iron*, is probably derived from two words, implying *bright* and *to melt**. Then, again, the names of compounds were even more mysterious than those applied to the metals, and never indicated the nature of the bodies concerned in their production. No information of this sort can be gained from such terms as the following:—" The Red Lyon, the Ravenous Wolf, the Red Eagle, Alocoph, and Panchymagogum minerali;" but modern chemistry has stripped these compounds of their high-sounding and uncouth titles, and conferred upon them plain and intelligible names: thus, the first, is a combination of oxygen and mercury, and accordingly it is called *peroxide of mercury;* the second, of sulphur and antimony, therefore, *sulphuret* of antimony; the third, of chlorine and gold, therefore, *chloride of gold;* the fourth, of muriatic acid and ammonia, therefore, *muriate of ammonia;* and the fifth, being a compound of chlorine and mercury, is accordingly called *protochloride of mercury.*

In pursuing our investigations, amongst the numerous elementary bodies and their almost infinite train of compounds, we find much to excite our wonder and admiration, whether we refer to our own artificial operations with them, or those which are carried on upon a more magnificent scale in the vast laboratory of nature.

Oxygen and nitrogen gases, when *mixed* together form the atmosphere, which is so congenial to the support of life; but, if they are chemically *combined*, they form a gas, which, when inhaled, produces most extraordinary intoxication: the same elements combined in other proportions, generate nitric acid, which is one of the most powerful and corrosive substances known, and in which silver, copper, mercury, iron, and many other metals will dissolve as readily as sugar does in water, forming a class of compounds called nitrates. Then again, oxygen and sulphur are both comparatively inert bodies, but they unite and form sulphuric acid, which is powerful and corrosive, and barely inferior to that just named, in its strong attraction for the metals. Combined with carbon, which is tasteless and inert, oxygen forms carbonic acid gas, which is highly inimical to animal life: this gas it is, that constitutes the poisonous atmosphere of the *Grotto del Cane*, and the no less fatal effluvia arising from charcoal fires; but it is present in large quantity in fermented liquors, and may be easily dissolved in water; it then constitutes the sparkling effervescence and grateful taste, of champagne, and seltzer water. United with the oxides of sodium or potassium, it forms the well-known substances carbonate of soda and carbonate of potash; with the oxides of lead and copper, it forms the white and green pigments which are so abundantly employed in painting. With the brilliant and inflammable metal calcium, oxygen unites and forms quicklime, which is extremely caustic and corrosive; this, combined with carbonic acid, forms chalk, or carbonate of lime, which has no corrosive property. Quicklime, united to sulphuric acid, produces the compound called Plaster of Paris, or sulphate of lime, also destitute of any corrosive property.

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* This name would almost lead us to imagine that the ancients knew cast-iron.

Chlorine unites with sodium, and forms common salt, or chloride of sodium, which has a pure saline taste and is an indispensable addition to our food. With hydrogen, chlorine produces muriatic acid; a very powerful poisonous substance.

Hydrogen and nitrogen are both tasteless and inodorous, but they unite and give birth to a gaseous body of intensely pungent taste and smell, namely, ammonia or volatile alkali; this united with muriatic acid produces a white solid body, having a bitter taste, and entirely devoid of smell, viz., muriate of ammonia; with sulphur, also inodorous, hydrogen combines to form a gaseous compound of the most disgusting and poisonous odour, namely, sulphuretted hydrogen, which exists in the well-known Harrowgate water, and is the cause of its remarkable peculiarity.

Hydrogen combines with carbon, and produces a gaseous compound, namely, carburetted hydrogen, which is now so extensively employed for the purposes of artificial illumination, and being of much greater levity than the atmosphere, it is accordingly employed for filling balloons, and thus enables the experimenter to carry on his researches amidst the trackless regions of the air.

Carbon, nitrogen, and hydrogen, combine and produce the deadly Prussic acid; many analogous instances might be cited, but the foregoing are sufficient for this general view.

The varied union of the elements in nature are calculated yet further to excite our astonishment, and awaken our interest to the manifold wonders of chemistry; whether we regard the infinite variety of rocks and strata, the contents of mineral veins, or the animated kingdoms, in these we learn the remarkable and wonderful fact, that very few elements are concerned. Oxygen, hydrogen, and carbon, are, generally speaking, all the elements which can be detected in every variety of vegetable matter; it heeds not how widely different the physical characters of vegetables may be, they *are all constituted* of the above ultimate elements: nitrogen *is sometimes found sparingly* superadded.

These are extraordinary instances of the extent and variety of chemical combinations; the reader will perhaps hardly credit, that the *white* page now before him, is a compound of charcoal and the elements of water, that the *black* characters which present this assertion, contain the *same* elements, and that no others can be found in sugar, gum, starch, resin, oil, or all the variety of vegetable acids. This, however, is a chemical fact, and the following is no less remarkable; all the softer solids of the animal body, consist of carbon, oxygen and hydrogen, combined in various proportions, and generally with abundance of nitrogen, whose presence appears to be one main distinction between animal and vegetable matter.

Nitrogen is found in *Fungi*, which thus form a sort of connecting link between a vegetable and animal matter; animal fat contains no nitrogen, and therefore, is allied to vegetable products.

The animal skĕleton is composed of oxygen, phosphorus and calcium, which elements by some recondite operation are absorbed into the living body and accreted into compact bone. But although we can prove, as far as our science goes, that the above are the elements, and the sole elements, existing in vegetable and animal matter, yet we cannot, by the utmost exertion of chemical skill cause them to unite artificially so as to produce even the most humble imitation of any organized substance; their union in nature is influenced by the mysterious and incomprehensible principle of vitality, towards which we can make no approaches.

DEFINITE PROPORTIONS

The union of the elementary substances and their compounds does not take place indiscriminately or in uncertain proportions; but the results are always constant and the composition of the varied products always the same. This fact presented itself at a very early period of scientific chemistry to those indefatigable analysts who busied themselves in ascertaining the composition of natural and artificial productions; it is only, however, within the last twenty years that the matter has been fully and perfectly investigated: but now, in consequence of our improved apparatus and methods of operating, it is reduced to principles of extreme simplicity and beauty, and known under the title of *The Theory of Definite Proportions.* Very many hypothetical views have been unnecessarily blended with it, chiefly by those whose mathematical knowledge is superior to their experimental skill: these certainly tend to a great extent to destroy the simplicity of the theory, which is far more usefully and satisfactorily considered as an independent collection of facts, all of which are the result of *experiment*. A brief, general, and popular view of the theory of definite proportions is a necessary appendage to the present essay: there is no mystery in the matter; and the juvenile student need not shrink from this page, under the idea that a long and abstract discussion is about to be commenced, for nothing more than a plain statement of a few incontrovertible facts will be presented to his notice.

Substances are found of perfect uniformity of composition as to their *ultimate elements*, whether presented to us by nature, or art,—at all times, *in all climates*, and by whatever chemists, their examination is undertaken: thus water, marble, salt, wood, sugar, bone, and muscle, present elements, combined in the same proportions now, as in the early dawn of chemistry; they are natural products. Vermilion, calomel, potash, aquafortis, Epsom salts, vinegar, and spirits of wine, are artificial products, and they present the elements they formerly did, and in the same proportions, by whatever process they are formed.

Hence arises the great certainty and precision of chemistry; for if its elements and their compounds were not always the same, no two results could ever exactly coincide with each other, and no agreement could exist between the experiments of chemists in this country and those upon the continent or other parts of the world. But now, if a chemist here, takes 54 parts by weight of aquafortis (nitric acid), and unites it with 48 parts by weight of potash (oxide of potassium), he produces exactly 102 parts by weight of nitre (nitrate of potash), and he knows that no other chemist can cause these two bodies to unite in any other proportions; therefore when he speaks of nitre, it is recognised as a body of definite composition by all other experimenters. Supposing that he makes a statement to the effect that 50 parts by weight of marble (carbonate of lime) have been united with 50 parts by weight of vinegar (dry acetic acid), and produced 100 parts by weight of *acetate of lime*, every chemist instantly knows that such result is erroneous and perfectly impossible; that, in this instance, 50 and 50 *cannot produce* 100, and therefore, that the statement is not entitled to credit; for 50 of marble and 50 of acetic acid only produce 78 of acetate of lime. This appears strange, but the solution of the matter is very simple, and will prove the importance of studying the theory of definite proportions.

Marble consists of 20 calcium + 8 oxygen = 28 lime or oxide of calcium, combined with 6 carbon + 16 oxygen = 22 carbonic acid, = 50 carbonate of lime. Carbonic acid is a volatile or gaseous body in its free and uncombined state, and as such it is expelled by the action of 50 parts of acetic acid; the whole of its 22 parts are lost, and the acetic acid taking its place with the 28 parts of lime, produces 78 of acetate of lime,—and this result is constant and definite at all times and under all circumstances. No person but a chemist would question the veracity of the statement that 50 of marble and 50 of acetic acid produce 100 of a new compound, or that a pint of spirits of wine and a pint of water produce a quart of diluted spirit; it has, however, been proved, that the first is erroneous, and the second is equally so, as will appear in its proper place in another paper. But now to examine the foundation of this theory, for these instances are taken from its superstructure.

It has been already stated (p. 295) that hydrogen is the *lightest* substance known, we therefore call it *unity* or 1; and in comparison with this standard the specific gravity of oxygen is 16, chlorine 36, iodine vapour 125, bromine vapour 76, nitrogen 14. It has also been stated (p. 297 and 301), that water consists of oxygen and hydrogen, muriatic acid of chlorine and hydrogen: let us now suppose that we attempt the formation of water by the union of its elements, let us take 1 of hydrogen and 16 *of oxygen, which* are *equal volumes*, mix them together in a *proper vessel, and pass an* electric spark through the mixture; the gases explode, water is produced, but exactly *half the original volume* of oxygen remains unaltered and uncombined, the other *half volume*, and the *entire volume of hydrogen*, having entered into union to produce water; hence the inevitable inference, that although the specific gravity of the gases are as 1 to 16, yet their *combining weights*, for that is the important part of the matter, their combining weights are as 1 to 8, producing 9 of water. If we carry on our researches concerning the union of oxygen with other elements, we find that 8 is the *smallest* proportion in which it will combine with any of them; hence 8 is called the *combining*, *representative*, *equivalent*, or *proportional number* of oxygen.

If we mix equal volumes of hydrogen and chlorine, whose relative weights are as 1 to 36, and expose them to sun-light, they combine with explosion, producing muriatic acid; they combine in *equal volumes*, 36 is therefore the representative number of chlorine, 37 that of muriatic acid, as in the last experiment 9 was the representative number of water; 1 of hydrogen combines with 125 iodine and 76 bromine, which are therefore their representative numbers, and those of the compounds produced are respectively 126 and 77. The theory of combining volumes is very beautiful, and an interesting adjunct to the theory of proportions, there being only one exception to it, which is the case of oxygen, it combines in a half volume; but it must be particularly remembered that the important part of matter is not that of *volumes*, but of *weights*. It is by weight that the chemist works, and that, too, with an exactness almost incredible; hence the numbers attached to the elements and compounds are those of their combining *weights*. The numbers attached to the elementary substances, at p. 294, are those of their *combining weights*, and are accordingly called *representative* or *proportional numbers*, which term is also applied to the numbers of compounds; thus 9, 37, 54, are the representative numbers of water, muriatic and nitric acid, &c.

But substances very frequently unite in more than one proportion, and when such is the case, each succeeding proportion is found to be some simple multiple of the first; this fact cannot be better illustrated than in regard to the compounds of oxygen with nitrogen, which are five in number. The first consists of 14 nitrogen + 8 oxygen = 22 nitrous oxide; the second, of 14 nitrogen + 16 = 30 nitric oxide; the third, of 14 + 24 = 38 hyponitrous acid; the fourth, of 14 + 32 = 46 nitrous acid; and the fifth, of 14 + 40 = 54 nitric acid.

Sulphur unites with oxygen in two proportions; in the first, 16 of sulphur + 16 oxygen = 32 sulphurous acid; in the second, 16 + 24 = 40 sulphuric acid.

We find these proportional numbers to hold good amongst the metals and their compounds; thus, 8 oxygen + 200 mercury = 208 protoxide mercury; 16 + 200 = 216 peroxide of mercury; 8 oxygen + 64 copper = 72 protoxide of copper; 16 + 64 = 80 peroxide of copper; 36 chlorine + 200 mercury = 236 protochloride of mercury; 72 + 200 = 272 perchloride of mercury.

If we wish to make a nitrate of a protoxide of a metal, for *instance*, *of* mercury, we must take 54, or one proportional of nitric *acid*, *and 208*, or one proportional of protoxide of mercury, which will produce 262 of protonitrate; but if a pernitrate is required, 108, or two proportionals of nitric acid, must be employed to correspond with the two proportionals of oxygen = 16, in one proportional of the peroxide = 216; thus we produce 324 of pernitrate of mercury. The proportional numbers of the two nitrates are, therefore, 262 and 324.

The same law holds good with other oxides, acids, metals, &c.

The theory of definite proportions is also applicable to animal and vegetable matters; thus bone is a phosphate of lime, whose composition is 12 phosphorus + 16 oxygen = 28 of phosphoric acid, + 8 oxygen + 20 calcium = 28 lime; the representative number of phosphate of lime is 28 + 28 = 56. Vinegar, or acetic acid, is a vegetable product, consisting of 2 hydrogen + 24 oxygen + 24 carbon = 50 acetic acid. The representative number of carbon is 6, consequently here 4 proportionals = 24 are concerned.

Our suspicions regarding the compound nature of the elements, chiefly rest upon the very high representative numbers which some of them bear in relation to hydrogen, which, from its levity and highly attenuated nature, approximates more to our notions of an element, using the term in its ancient acceptation *of absolute simplicity*, than any other form of matter with which we are acquainted. Oxygen and nitrogen are gross in comparison with it; iodine, columbium, gold, and uranium, amazingly so: reasoning from analogy, their high numbers denote a compound nature; but this, let it be remembered, is a *mere hypothesis*, and cannot yet be demonstrated as a chemical fact.

The above examples of the theory of definite proportions must suffice for the present, its further discussion will be undertaken, when we come to the particular consideration of simple and complex affinities; indeed, the elements, nomenclature, and theory of definite proportions are too important to be justly treated in an occasional composition of a few brief pages; and the views which have been taken are designed rather to excite feelings of interest concerning them than to give minute information.

ON THE NATURE, EVIDENCE, AND ADVANTAGES OF THE INDUCTIVE PHILOSOPHY.

III.

THERE is one grand, fundamental principle, without which no induction of laws from particular instances, no generalizations of individual truths, no regular or systematic study of nature, could ever proceed: and this is our conviction of a permanence and uniformity in the order of natural things: our belief that that which has happened in succession for days and years past, will, under the same circumstances, continue to happen for time to come: our persuasion that what so takes place in one instance, in one place, will and does take place, under the same conditions, in all other instances, and in all other places. We suppose, that is, that nature *is so constituted*, that there exists *some* principle of undeviating regularity in the connexion of qualities and properties, of causes and effects, even though we should fail in always tracing it.

This belief undoubtedly exists and operates in very different degrees in different minds. But a share of it, at least, is so universal, that some metaphysicians have been disposed to regard it as constituting one of the inherent principles of our nature. Thus, the most ignorant person infers that the sun will rise to-morrow, and for succeeding days and years, because he has so regularly witnessed it before; and that a stone falls to the ground as constantly in America as in Europe.

In the limited form in which we commonly notice the operation of this sort of intuitive persuasion, it certainly does not amount to anything like a philosophical conviction of the uniformity of natural causes. It is, doubtless, restricted to certain isolated classes of facts, which in all their circumstances are constantly falling under the observation. In those limited instances, the individual, perhaps, relies on their recurrence from mere habit, which probably does not produce in his mind any general belief that other events beyond the limits of his observation are regulated by any like constant uniformity. Nor when the idea is suggested is he able to perceive the force of the inference from analogy; but, probably, imagines all things beyond the precise extent of his observation to be destitute of any determinate order, and the course of events in general, either under the dominion of arbitrary agency, or abandoned to chance or blind destiny.

In proportion, however, as the mind is more cultivated, and man accustomed to reflect and reason on the objects continually presented to his senses, he is naturally, and even unconsciously, led to enlarge his persuasion of the future recurrence of natural phenomena, in the same order in which he has several times witnessed it. This persuasion easily extends itself to a great variety of particular instances, in which its correctness is soon verified by observation. The same habitual judgment thus gains strength by every hour's experience. The confidence with which the mind calculates, as it were, upon the permanence of a certain order in physical events, increases with rapidly-accumulating force; and the improvement of the faculties by study, and the enlargement of our stores of information from wider observation of physical facts, soon begins to induce the habit of extending our persuasion of the uniformity of natural causes, beyond the mere bounds of familiar phenomena, to those which are placed out of our immediate examination, but which we come naturally to imagine must be regulated by a like constancy.

Founded, then, on the natural constitution of the human mind, confirmed by daily experience, and verified by every advance in the accurate study both of mental and material phenomena, the belief in the existence of this uniformity becomes, in fact, the basis of all acquisition of knowledge, and enables us, without hesitation, to advance in our conclusions from the known to the unknown, from truths actually before us and within our reach, to those which may be hidden from us, or utterly beyond the limits of sensible experience.

The belief in the uniformity and permanence of natural order, combined with, and perhaps dependent on, the tendency of the human mind to generalize its observations, unite to supply, as it were, the rude materials of philosophic investigation. But it is further necessary that they should be skilfully wrought and fashioned before they can be of any use. We have then further to inquire how this is to be done; and we shall find that the models by which we must work, are to be found in the careful and extended study of already established natural relations.

The rules by which we are to be guided in advancing to these generalizations of observed physical relations, must be those derived from the careful study and comparison of such generalizations previously confirmed in other corresponding instances.

It will be to little purpose that we are persuaded of the existence of *some* uniformity in natural laws, unless we have this guide to assist in tracing what the principle of uniformity is in any particular case. Without such assistance, we may go on collecting and observing a vast number of facts, and yet arrive at no conclusions, or only at such as are altogether empty and visionary.

As we have already remarked, that merely to affirm what we observe in common of a number of individuals, all of whom are before us, is hardly worthy the name of an induction, so it is a violation of all just induction to infer a general property from too limited a number of instances. But what constitutes the *sufficient* number of instances must depend on the nature of the case, and the experience and power of judgment possessed by the inquirer.

And if we fall into the error of *too small* an induction, the usual cause of such error is rather that the induction is wanting in a just principle of probability in our first conjecture, or that we have proceeded on the supposition of a wrong sort of relation. It is this which has commonly much more to do with the justness of our conclusion than the mere *number* of instances collected. And, on the other hand, it often happens that a very few instances, or even almost a single instance, have been admitted without question as a sufficient verification: but this has depended entirely on the justness of the assumed relation.

We will illustrate these remarks by a few examples, both of successful and unsuccessful inductions, taken from different departments of science.

1. As we descend in mines, it has been found that the temperature uniformly increases. Hence it has been inferred as an inductive conclusion, that the earth is universally hotter, the lower we descend into its interior, and that there exists a source of central heat of great intensity.

This conclusion is objected to by some philosophers; and it is a fair question, Why we should infer that the temperature goes on *perpetually* increasing to the centre, because it may do so within a limited depth?

The regular increase of temperature with the depth has been carefully ascertained in various parts of the globe; the presumption that it increases in all parts as we descend, is, therefore, not altogether without foundation. But the question cannot be considered as positively decided. It clearly depends on whether the number of places in which observations have been made, be as yet sufficient to justify the *universality* of the conclusion: and whether there may not be local sources of heat at comparatively small depths, such as those which produce volcanoes, which may be sufficient to account for the effects. There is a want of any fair ground of antecedent probability in favour of the hypothesis to guide us.

2. Newton, on passing a ray of light through a prism of glass, found it separated into coloured rays; and measuring the proportion in which it is thus spread out, or "dispersed," announced that proportion as the general law of prismatic dispersion.

Dr. Lucas repeated the experiment; but assigned a *much less proportion* as the law. Both parties positively maintained the correctness of their respective conclusions. But they had both argued on a faulty ground of induction: they had each taken for granted that their prisms ought to act equally on light. The fact was, they had used different sorts of glass, which vary considerably in dispersive power.

This is remarkable as one of the very few instances in which Newton failed in an induction; but such failures are instructive; for we learn to observe the reason of the error. It was manifestly from neglecting to consider, in this case, what *probability* there would be, *previous to trial*, that different sorts of glass should possess the same dispersive power.

3. On the other hand, Newton's capital result that "to the same ray ever belongs the same refrangibility" (the media being the same), is a conclusion, indeed, of a most general nature, and which universal experience has amply confirmed, but it was founded on a very limited induction derived from prismatic experiments with, at most, three or four different media.

4. The early history of Astronomy is full of examples of the compatibility of accumulated observation with the want of satisfactory induction. The ancient astronomers were indefatigable in the diligence with which they amassed observations. But they constructed out of them no theory which could attain a real permanence. The system of Ptolemy sufficed to a certain extent to represent the observed motions of the planets. The advance in accuracy of observations, however, soon required *corresponding* improvements in the system; which was obliged to be *modified to* accord with them: but, at length, the immense complexity

introduced by the cycles and epicycles which were necessary to account for the apparent motions, began to induce a persuasion that such complication could not be the real law of nature: juster principles were therefore to be sought. No astronomer ever laboured more sedulously in making and recording observations, than Tycho Brahe. But though persuaded of the insufficiency of the Ptolemaic hypothesis, he did not succeed in constructing a better: not from deficiency of facts, but from his strangely-erroneous assumption of a guiding theoretical principle.

Kepler worked upon Tycho's materials. The labour which he bestowed on calculation was absolutely incredible. But theory after theory was adopted and rejected, because he had not any other guide than random conjecture, and nothing but the accurate calculation of every detail could suffice to put those conjectures to the test. He had not lighted on any happy ground of *antecedent probability*. When, however, at last, he did seize upon the true law of nature, the numerical verification was perfect and decisive; and when thus established in the single instance of the planet Mars, it is extremely instructive to observe the rapidity and facility with which the inference was extended to the whole solar system.

When the laws of the motion of one planet were established, a single conjecture sufficed to point out, with the highest degree of probability, the laws of all the other planetary orbits: and a single calculation to verify it. The difference was, that there was now a ground of antecedent probability. A presumption of a guiding resemblance, which (though perhaps no precise reason could be assigned for it) was yet such as to leave no doubt that it had some foundation in nature.

Thus, then, it is manifest, that to possess some reasonable ground of antecedent probability, as a guide to our conclusion, is absolutely essential to physical *induction*. And we cannot employ the term correctly in its higher sense, (as referring to anything above a mere collection of instances,) without meaning to include specially the notion of a fair presumption of some relation, in virtue of which we can argue from the known to the unknown; and infer that those cases which we do not see, are probably connected with those which we do. This constitutes one most essential characteristic of the inductive process; and without it, assuredly we can never advance to a substantial conclusion. We must always, then, consider the inductive method as referring, not merely to the accumulation of instances, but as *involving the idea of some presiding conception, some guiding principle, of presumed connexion and probable relation* between the facts on which we are reasoning.

In replying, then, to the inquiry, What constitutes the ground of antecedent probability, so essential to a good induction? it will be almost apparent, from the examples already cited, that the main ground is that afforded by the *comparison of one class of phenomena with another:* the perception of a *parallelism* in their respective conditions: the existence of an ANALOGY between them.

The success, then, with which induction may be carried on, depends on the just appreciation of such trains of analogy. This can only be attained by a *habit of* cautiously comparing one presumed generalization *with already established* laws. One induction must be the guide to another. We must seek to interpret nature in accordance with her own principles already displayed. Every real natural truth, we may be assured, will be in harmony with other parts of the great series and scale of natural truth. With this our hypothesis must be in accordance; to ascertain and verify such accordance is the aim of the true philosopher; and it is entirely on the justness with which it is preserved that the whole truth and success of induction depends.

Observation exhibits a certain law or relation among a particular class of facts. This suggests to the mind of the philosopher the probability of the same relation in some parallel class of facts. The relation being firmly established in one set of instances, he feels satisfied with even a slight indication of it in the other. The conviction of its probability once formed, a very few cases adduced serve to verify it. The experience of instances actually tried, leads to the expectation of analogous results in cases untried. But the essential point is the real *parallelism* of the cases. The hypothesis will be philosophical or not, according to the extent and justness of the comparison which has suggested it.

For example (1.) Experiment had shown that electricity in a high state of tension discharges itself with a flash and a report. Lightning and thunder exhibited an instance of a flash and a report. The atmosphere was known to be susceptible of electrical influence. All this had been ascertained, but no relation had been established between the cases. Other causes might possibly produce a flash and a report. But the analogy of electricity presented itself strongly to the philosophic mind of Franklin. By the string of a kite, as a conductor, he brought down the electricity of the clouds, which, on its arrival at the ground, was regularly discharged with sparks, and the analogy converted into an identity.

(2.) Every one had been accustomed for ages, before the time of Newton, to observe, that bodies fall to the ground as soon as support is withdrawn. They were equally familiar with the fact, that the moon circulates periodically about the earth. But no one ever perceived any relation or imaginable connexion between these two classes of facts. Nay, the peripatetics, maintaining that the heavenly motions were of an essentially different kind from the terrestrial, led men to the belief that these two cases could not possibly have any common relation.

The penetrating mind of Newton, however, instantly perceived a connexion between them. He considered that a body launched into space would continue to move off in a straight line, unless made to deviate from that path by the action of some other cause. The moon does not go off in a rectilinear path, but has her course continually *bent* from such direction into a curvilinear orbit round the earth, and the degree in which it is thus bent or the amount of deviation from the straight course, is in fact so much of a real *fall* towards the earth: the moon is actually falling like a stone: and the amount of its fall can be measured; since astronomical observation has given the size and form of its orbit and the rapidity of its motion. Also the amount of the fall of a stone near the earth's surface is known. It becomes a matter of calculation to compare them. Newton made the comparison, and found the two effects precisely in the inverse proportion of the squares of the distances from the earth's centre. This was the precise proportion which would agree with the supposition of that law of central force, which, on abstract mechanical principles, ought to give rise to elliptic orbits, and to certain relations expressed by numerical laws between the magnitudes of those orbits and the motions in them. These were the very same as those numerical relations had been found by Kepler long before to subsist in the planetary revolutions.

Thus the single circumstance of the *analogy* between the moon's motion and that of a stone falling to the ground, sufficed as a clue to the whole system of planetary motions, and the establishment of the principle of universal gravitation.

(3.) Physical philosophers had been long seeking to establish (what there was every reason to suspect,) the existence of at least a close connexion, if not absolute identity, between electricity, galvanism, and magnetism. There were many points of resemblance in what was known of the nature of those agents; experiments had been multiplied, and many curious facts, and results had been accumulated. But all this *collection of facts* had not afforded a real *induction*. And the reason was, that the inquirers had been guided either by no principle of analogy, or by such as was incorrect.

The most powerful electric forces had been resorted to; but no evolution of galvanic influence, no shock, however strong, would affect the magnetic needle. Experimenters were accustomed to witness the most intense electric action when the current was broken, or the accumulated power discharged; here, therefore, they expected to find the greatest effect of a magnetic kind.

But the modes of action with which philosophers had been previously acquainted, were, in fact, of a kind offering no analogy to those concerned in the cases in question. This, however, was not perceived, till Œrsted discovered the real point of connexion of electricity and magnetism. He succeeded, by a very slight change in the arrangement from that with which his predecessors had been so long and so fruitlessly working. By using an *unbroken* galvanic circuit, he instantly found an influence on the magnetic needle: not by violent concentration of forces but by a peculiar diffusion of them. And the whole system of action by transverse currents was almost immediately developed and followed out into all its correlative trains of consequences.

(4.) Newton published his *Principia* before any instance of the periodical return of a comet had been established, or even imagined. Yet, on comparing the masses of these bodies and their distances with those of the planets, he caught an analogy, and did not hesitate to speak positively of their describing orbits about the sun, and to recommend to future astronomers to verify their returns by comparison of observations. It is superfluous to notice how completely this idea has been borne out by subsequent discoveries*.

(5.) In the extension of the law of gravitation from the fall of a stone on the earth to the motions of the most distant planet or the most erratic comet, we have a remarkable instance where a conclusion is made from effects which we observe near us, to those of the same kind which are produced in the remotest regions of *space*. Let us compare this with a parallel case in *time*. We observe the daily formation of rounded pebbles by the action of the waves on fragments of rock on the sea-shore; and we find the incessant continuance of that action for a long time give rise to accumulated beds of shingle.

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* *For example*, see our last Number, p. 250.

Now, over large tracts of land, at considerable elevations above the sea, we find immense beds of pebbles presenting precisely the same appearance of rolled and rounded fragments, as those we now observe in the progress of formation in the sea. It is, then, by the same process of reasoning which connects the gravitation of a stone with that of the moon, or the remotest planet or comet, that we connect the formation of beds of pebbles at the present day with that of similar beds in ages of remote antiquity, when the present dry land formed the bottom of the ocean; or, rather, was gradually emerging from it, through such a long succession of ages as would alone suffice for the production of the immense beds of rolled gravel which we find deposited over a large part of the surface of the globe.

Philosophical induction, then, proceeds mainly by seizing upon analogies between known orders of facts, known relations of cause and effect, and cases where the existence of such relations is unknown, but where the circumstances render it probable that they also subsist. Such circumstances, perhaps quite casual and unimportant in the eyes of the ordinary observer, suggest in an instant, to the practised mind of the philosophical inquirer, a train of relations in which the analogy is maintained. He proceeds to verify his idea: a single experimental instance often suffices to confirm it; and at most, a very few repetitions and variations in the circumstances and conditions, satisfy him that his analogy is correct, and the uniformity of the law by which physical action is determined becomes established.

In fact, so essential to induction is the dependence on analogy, that in the very use of the terms, "observation," "experience," and the like, by many writers, to describe the grounds of our belief in physical events, it is evident that they mean to include *essentially* the reference to *analogy*, and not barely to facts actually witnessed. Unless this be the case, indeed, their meaning would in some cases be involved in absurdity and contradiction.

Thus, then, the most important part of the process of induction consists in seizing upon the probable connecting relation by which we can extend what we observe in a few cases to all. In proportion to the justness of this assumption, and the correctness of our judgment in tracing and adopting it, will the induction be successful. The methods by which a facility in discovering such relations, and a readiness in forming such judgment, may be attained and improved, are precisely the objects principally to be kept in view by the philosophical student who would prepare himself for the work of interpreting the phenomena of the natural world. The analogies to be pursued must be those suggested from already-ascertained laws and relations. This, in proportion to the extent of the inquirer's previous knowledge of such relations subsisting in other parts of nature, will be his means of guidance to a correct train of inference in that before him.

And he who has, even to a limited extent, been led to observe the connexion between one class of physical truths and another, will almost unconsciously acquire a tendency to perceive such relations among the facts continually presented to him. The truth of the remark to which we have been thus led is amply confirmed by the history of philosophical discovery.

In point of fact, discoveries, commonly termed inductive, have very seldom been really attained by the mere process of amassing collections of individual facts. It has been almost invariably the case that hypothesis has preceded observation; and that the discoverer has in truth only verified, by an appeal to experiment, the general theory which he had already imagined. The happy selection of such hypotheses is that which characterizes, and in fact constitutes, philosophical genius. And a just appreciation of the use of such imaginary provisional assumptions, eminently distinguishes the rational inquirer from the speculative visionary. The true philosopher neither discards hypothesis on the one hand, nor yields himself up to it on the other; but rates it at its proper value, and turns it to its legitimate use. He is always ready to reject an assumed theory the moment he finds it unsupported by fact: but if it be once duly substantiated, to adopt it, and be prepared to follow it out into all its legitimate consequences, however at variance with received notions,—however contrary to established prejudices,—however opposed to the prepossessions, the bigotry, the cherished delusions of mankind.

And the more extensive his acquaintance with nature, the more firmly is he impressed with the belief that some such relation must subsist in all cases, however limited a portion of it he may be able actually to trace. And it is by the exercise of an unusual skill in this way, that the greatest philosophers have been able to achieve their triumphs in the reduction of facts under the dominion of general laws.

But important as these natural analogies are to the philosopher, they are yet of a nature which renders it difficult to make them generally appreciated: and, unless by actual and attentive study of physical science, it would probably be a hopeless task to attempt to convey an adequate conception of the irresistible claim to acceptance with which they present themselves to the mind of a person even moderately versed in such inquiries. Yet they are, in fact, no more than extensions of the very same elements of thought, which seem implanted in our nature; by which all our acquaintance with sensible objects is, in the first instance, acquired; and by which we are continually and unconsciously storing our minds with that knowledge, which is so necessary for all the purposes of our existence;—those natural persuasions upon which all uniform convictions, and all consistent conduct is based;—and without which life would be a continued state of infancy.

But it is not, perhaps, until we come to contemplate natural phenomena, exhibited in the form of numerical results, and find those data reducible to mathematical laws, that we fully appreciate the reality and exactness of that uniformity by which all nature works. The coincidence with such laws, is that which, above all others, impresses us with the conviction of invariable order and uniformity pervading the material universe.

We find this, in the first instance, in the reduction of vast collections of observed numerical results, under simple mathematical laws. But the more extended application of mathematical analysis powerfully augments the impression produced on our minds by the conspiring inductions, and corroborating generalizations, of purely physical investigations. From some one very simple, remote, and abstract datum, obtained from elementary physical facts, we often proceed by purely mathematical reasoning, perhaps through a long and intricate deduction, which at length brings us to the conclusion, that, under certain conditions, a particular kind of action ought to take place; and even the precise amount of its effects ought to be such as are given by a certain analytical expression. The results of observation exactly accord with these deductions; and even the minutest variations in the effects are exactly represented by calculation from the formula of theory.

We have occasionally singular exemplifications of the existence of recondite principles of analogy, in the coincidence of phenomena with the symbolical indications of mathematical analysis. A mathematical formula is found, which expresses the law of a certain class of phenomena. The analytical language of symbols admits, perhaps, of certain changes, or embraces certain cases, not at all contemplated in the first numerical establishment of the law; but dependent purely upon abstract algebraical rules and transformations. These symbolical changes shall be found to have physical cases exactly corresponding to them.

Of this, we have the most striking instances in optics. We may cite the simple case of the law of refraction; whence, by a mere abstract algebraical change, the substitution for the numerical refractive index, of the fictitious abstraction (-1), we obtain another formula, which is no other than the law of reflection, and a whole series of formulas, expressing all the consequences of that law, corresponding to those of the law of refraction.

In the higher departments of physical optics, the same thing has been most surprisingly exemplified. We need only cite the marvellous prediction of the conversion of plane into circular polarization of light, by two internal reflections in glass, made and verified by M. Fresnel, entirely upon the strength of certain mechanical and mathematical analogies. "A conclusion," (as Professor Forbes justly remarks,) "which no general acuteness could have foreseen; and which was founded on the mere analogy of certain interpretations of imaginary expressions. The mere reasoner about phenomena could never have arrived at the result,—the mere mathematician would have repudiated a deduction founded upon analogy alone."—(On "Polarization of Heat," *Edinb. Trans.* Vol. xiii.)

BOTANICAL RAMBLES IN THE VICINITY OF DOVOR.

No. I.

The increasing taste for botanical pursuits, and the wish, now so generally shown, of gaining an acquaintance with our native Flora, has induced me to draw up the following account of a few rambles in search of plants, which I made in the vicinity of Dovor, last autumn, and in the autumn of 1833.

Dovor, with its magnificent and far-famed cliffs, its venerable and imposing castle, its sea-views, so beautiful and full of animation, is very justly admired, and a numerous set of visiters annually assemble there in the summer and autumn. To many of these, I hope my rambles will be useful, if not interesting, and will serve to point out certain spots, where, during their walks, they may find many of our pretty and interesting native plants.

In visiting a new neighbourhood, I have often had to regret the want of such an aid,—particularly when my time has been short;—and, supposing others may have the same feelings, I with less hesitation take up this subject, which may appear to some so trifling.

I do not aim at a correct list of all the plants found in the vicinity of Dovor, many must have escaped me,—and the early spring plants I have had no opportunity of observing;—however, I hope that there will be a sufficient number noticed, to give a tolerable idea of the character of the flora.—For further information on the plants of this part of the coast, G. E. Smith's *Catalogue of the Plants of South Kent* may be consulted, and the localities of many very interesting and rare plants will be found there detailed. I will here observe, that many of the more common plants will not be mentioned, as the list, by so doing, would only be enlarged, without rendering any service to the reader. In my names I follow Hooker,—considering his *British Flora* to be more generally used than any other at the present time. His work may be referred to for ascertaining the time of flowering, or other facts the reader may require, as I shall merely give the names of the plants, with here and there a remark.

The best method, I think, of pointing out the localities, will be to request the reader to go over the ground again with me; but before we proceed on our first ramble I may be perhaps allowed to state, for the information of those who do not know the nature of the soil and country about Dovor, that it is entirely chalk, forming, inland, a series of rounded undulations and downy steeps, and, towards the sea, breaking into a line of fine and bold cliffs. This line is intersected, at Dovor, by a valley, in which the town, and several small villages, are situated, with the road to Canterbury running along a great part of its length. To the eastward of Dovor, the cliffs continue in one unbroken line for about three miles; then they sink to a mere bank at St. Margaret's, but immediately afterwards show themselves again. To the westward, the line, commencing at Archcliff Fort, continues unbroken to Lydden-spout, a distance of about two and a half miles; here it sweeps inland, at a small distance from the sea, and proceeds on towards Folkstone, almost disappearing before it reaches *that town*. By the sweeping of the cliffs inland, at *Lydden-spout*, a most beautiful but miniature undercliff is formed, which

I consider the most lovely spot in the neighbourhood, and one possessing peculiar attractions to the botanist, from the number of interesting plants found there. Beautiful and secluded as this spot is, still it is condemned to be spoiled by a railroad, which will pass along its whole length, and ruin it for ever. Advocate as I am for railroads, it is with deep regret I see this favourite spot about to be destroyed. At the foot of the cliffs, in many places, there are sloping banks, some of considerable magnitude, formed, apparently, by the gradual crumbling away of the face of the cliff. In other places, the sea washes up to the face of the cliff at high tide, leaving no kind of communication, and, therefore, rendering the passing of such spots about high-water somewhat dangerous.

We will now prepare for a ramble, and our first expedition shall be from the town, under the castle-cliffs, to the zig-zag path leading up to the station-house, and thence we will return home along the top of the cliffs and through the castle-meadow.

As soon as we get clear of the houses, we shall come to a series of sloping banks with the cliff rising perpendicularly behind them. This spot is quite a botanic garden; and, with due search, we shall discover the following plants, some of which, however, will require a little climbing to gather, as they grow high up on the banks, near the face of the cliff. Hard Meadow Grass (*Poa rigida*), Devil's-bit Scabious (*Scabiosa succisa*), Field Knautia (*Knautia arvensis*), Yellow Bed-straw (*Galium verum*), Hoary Plantain (*Plantago media*), Bucks'-horn Plantain (*Plantago coronopus*), Wall Pellitory (*Parietaria officinalis*), Common Vipers' Bugloss (*Echium vulgare*), Common Centaury, (*Erythræa centaurium*), Autumnal Gentian (*Gentiana amarella*), Common Burnet Saxifrage, (*Pimpinella saxifraga*), Samphire (*Crithmum maritimum*), Common Wild Parsnep (*Pastinaca sativa*), Sea Beet (*Beta maritima*), Upright Spiked Thrift (*Statice spathulata*), Perfoliate Yellow-wort (*Chlora perfoliata*), Great Hairy Willow-herb (*Epilobium hirsutum*), Small-flowered Hairy Willow-herb (*Epilobium parviflorum*), Nottingham Catchfly (*Silene nutans*), Sea Spurrey Sandwort (*Arenaria marina*), Dyer's Rocket (*Reseda luteola*), Bare Rocket, or Wild Mignonette (*Reseda lutea*), Yellow Horned Poppy (*Glaucium luteum*), Common Marjoram (*Origanum vulgare*), Self-heal (*Prunella vulgaris*), Common Eye-bright (*Euphrasia officinalis*), Common Vervain (*Verbena officinalis*), Clove-scented Broom-rape? (*Orobanche caryophyllacea*), Sea Cabbage (*Brassica oleracea*), Fine-leaved Mustard (*Sinapis tenuifolius*), Sand Mustand (*Sinapis muralis*), Common Kidney Vetch (*Anthyllis vulneraria*), Hawk-weed Picris (*Picris hieracioides*), Dwarf Plume-thistle (*Cnicus acaulis*), Common Hemp-agrimony (*Eupatorium cannabinum*), Ploughman's Spikenard (*Conyza squarrosa*), Common Golden-rod (*Solidago virgaurea*), Greater Knapweed (*Centaurea scabiosa*), Spreading Halbert-leaved Orache (*Atriplex patula*).

Among the foregoing plants I may remark that the common vipers' bugloss sometimes grows here in a very unusual way, the leaves being densely clothed with long hair-like bristles, and the spikes of flowers apparently abortive, and consisting of a mass of whitish-green bristles, giving them somewhat the appearance of those excrescences on the rose-tree, caused by the puncture of an insect. The variety of leaf exhibited by the common burnet saxifrage, will often puzzle a young botanist, and shows how the leaves of the same plant are sometimes liable to variation. In the leaflets of this plant all the intermediate gradations between nearly entire and pinnatifid, are to be observed; and the fact would be clearly demonstrated by a dozen specimens gathered from the sloping banks just spoken of. The sea-cabbage is generally supposed to be the parent of all our garden-cabbages; if so, the effect of cultivation is very curious, seeing the number of well-marked varieties now cultivated, which seem to bear no relationship to each other. The sand-mustard grows sometimes in the very loosest chalk-rubbish, where it would appear impossible for the plant to find any nourishment; but its long white roots penetrating deep into the mass, find for it a sufficiency, as the plants seem to thrive well. The common hemp-agrimony is sometimes found here with white flowers, or white with a greenish cast. To the clove-scented broom-rape I have placed a ?, as I do not feel quite certain about the plant, my specimens having been very far advanced when gathered. The samphire grows mostly on the perpendicular face of the cliff, where only the regularly-equipped samphire-gatherers can get it; but a few stray specimens may, at times, be found on the banks below, just sufficient to allow the botanist specimens for his herbal.

These sloping banks, after continuing on for some distance, cease, and the sea flows up to the foot of the cliff, stopping the communication for a few hundred yards at high water. Just in this spot, on the face of the cliff, we shall find a very interesting little plant, the smooth sea-heath (*Frankenia lævis*), growing a few feet above high-water mark. In one place it abounds, but will require some search before it be detected. I had often passed this part of the cliff before my attention was drawn to it, the plant is of such humble growth. Further on, we shall come to another series of sloping banks, very similar in their flora to those we have passed. Here we shall notice, however, in addition, some large beds of privet (*Ligustrum vulgare*), and observe the upright spike-thrift, sometimes called sea-lavender, growing in peculiar luxuriance, and showing itself off to great advantage. It is a beautiful plant, the pride of these cliffs. We shall now have reached the zig-zag path, leading up to the station-house, and as we mount we shall find the wild madder (*Rubia peregrina*), and the tufted horse-shoe vetch (*Hippocrepis comosa*), the former growing near the beginning of the ascent, and the latter near the top. We are now on the downs, and turning our faces towards Dovor, we will keep near the edge of the cliff, on a belt of greensward, which has been left between the cultivated ground and the cliff's impassable barrier. This belt produces common Quaking-grass (*Briza media*), small Scabious (*Scabiosa columbaria*), purple Medick or Lucerne (*Medicago sativa*), common Carline Thistle (*Carlina vulgaris*), blue Flea-bane (*Erigeron acris*), and the deliciously-perfumed Ladies' Tresses (*Neottia spiralis*). The Ladies' Tresses grow on that part of the belt near the castle-meadow, and merit attention on account of their unpretending appearance and rich perfume, as well as belonging to a very interesting family of plants.

On the cultivated ground we shall have no trouble in finding the blue Sherardia (*Sherardia arvensis*), Hemp-nettle (Galeopsis ladanum), *common Basil Thyme* (*Acinos vulgaris*), round-leaved Toad-[illegible] (Linaria *spuria*), sharp-pointed Toad-flax (*Linaria elatine*), and here and there patches of common Saintfoin (*Onobrychis sativa*), which can scarcely be called wild, as they have most probably resulted from cultivation.

The belt of greensward above mentioned, will lead us to the castle-meadow, a very beautiful spot, from whence the castle is seen to great advantage. Here, on the very edge of the cliff, are a few bushes of Burnet-leaved Rose (*Rosa spinosissima*), and about the hedges we shall notice great Mullein (*Verbascum Thapsus*), mealy Guelder-rose (*Viburnum lantana*), common Calamint (*Calaminta vulgaris*), and the beautiful heath False Brome-grass (*Brachypodium pinnatum*); the latter growing in considerable abundance here, and in several other places in the immediate neighbourhood.

I shall now say adieu for the present, and wish the reader a pleasant walk down the old Deal road into the town. W. W. S.

DESCRIPTION OF

AN APPARATUS FOR INDICATING THE INTERNAL TEMPERATURE OF ANIMALS AND VEGETABLES; AND OF THE MODE OF USING IT.

II.

It has already been stated that probes introduced into the bodies of animals do not indicate the exact temperature of the parts examined, unless the loss of heat which the conduction of the probe produces, be instantly renewed. Though this condition has been found to be obtained when the probes are of very small diameter, it may be useful to examine to what extent the temperature of a muscle, a tissue, or any other organ, is modified by the temporary inflammation which may follow the introduction of a foreign body.

It might, at first, be supposed that if any part of the heat indicated by the thermo-electric effect, could be attributed to irritation produced by the introduction of the probe, its quantity would be increased in proportion as the size of the instrument might be enlarged; that this is not the fact the following experiments will demonstrate.

The soldered-joints, iron and copper, of two probes, $\frac{1}{50}$ of an inch in diameter, having been placed, one in the mouth of a person aged 20, the other in the biceps muscle of the arm of another young man, a deviation of 8° was observed on the thermo-multiplier in favour of the muscle; now as it was known that 1° of deviation corresponded to 0°.2 Fahr., there was evidently a superiority of 1°.6 Fahr. in the temperature of the muscle; when probes of double the diameter, or $\frac{1}{25}$ of an inch in diameter, were used, the deviation (and consequently the temperature) was precisely the same; and with probes still larger, this amount of deviation did not vary during ten minutes. It is evident from these experiments, that the presence of probes in muscles and other parts of the body does not sensibly affect the temperature of them, and it is by no means difficult to conceive the reason,—the probes, on their introduction into a muscle, &c., simply effect a separation of the parts, and as they produce no derangement which destroys organic structure, they excite no change of temperature.

The apparatus, mode, and effect of experimenting, having been described, it will not be necessary to give more than the results of some examinations which were made by these means, on three persons of different ages (designated by A, B, C, in the tables which follow), on a carp, and on several dogs.

FIRST SET OF EXPERIMENTS.

Temperature of the Atmosphere 53°·6 Fahr.

	Parts examined	*Temp. Fahr.*	*Difference.*
I. A.	aged 20.		
	1. Biceps Muscle of the Arm	97°·75	3°·29
	2. Adjacent cellular Tissue	94·46	
	3. Mouth	98·24	
II. B.	aged 20.		
	4. Biceps Muscle of the Arm	98·29	2·48
	5. Adjacent cellular Tissue	95·81	
	6. Mouth	98·06	
III. C.	aged 55.		
	7. Biceps Muscle of the Arm	98·19	2·60
	8. Adjacent cellular Tissue	95·59	
	9. Mouth	98·60	
IV.	Black Dog.		
	Flexor Muscle of the Thigh	101·12	2·52
	10. Cellular Tissue of the Neck	98·60	
	11. Abdomen	101·30	
	12. Stomach	101·12	
V.	Another Dog.		
	13. Muscle of the Thigh	100·40	
	14. Stomach	98·60	
	15. Abdomen	100·58	

SECOND SET OF EXPERIMENTS.

VI. B.	aged 20.		
	16. Biceps	98°·29	2°·25
	17. Cellular Tissue	96·04	
	18. Calf of the Leg	98·42	
	19. Mouth	98.60	
VII. C.	aged 55.		
	20. Biceps	98·42	2.83
	21. Cellular Tissue	95·59	
VIII.	Black Dog, same as Case IV.		
	22. Muscle of the Thigh	101·48	

THIRD SET OF EXPERIMENTS.

IX. A.	aged 20.		
	23. Mouth	98°·51	
X. B.	aged 20.		
	24. Mouth	98·33	
	25. Do. measured by the Thermometer	98·60	
	26. Biceps	98·78	2°·92
	27. Cellular Tissue	95·86	
XI.	Carp. (*Cyprinus carpio.*)		
	28. Various Parts	56·3	0·9
	29. Water	55·4	

FOURTH SET OF EXPERIMENTS.

Probes used, those of the second kind; see fig. 4, 5, 6, p 261.

	Parts examined.	*Depth to which the Probe was inserted.*	*Temp. Fahr.*	*Difference.*
XII. B.	aged 20.	in.		
	30. Biceps	1·2	98°·15	
	31. Muscles of the Calf of the Leg	1·6	98·15	4°·05
	32. Adjacent cellular Tissue	0·4	94·10	
	33. The great pectoral Muscle	1·6	98·15	4·05
	34. Adjacent cellular Tissue	0·4	94·10	

XIII. Dog, English, of an average size.			
35. The great pectoral Muscle	1·6	100·85	1·35
36. Cellular Tissue	0·4	99·50	
XIV. B. aged 20.			
37. Biceps	1·4	97·70	3·60
38. Cellular Tissue		99·10	
XV. Dog.			
39. Muscle of the Thigh		101·30	0·99
40. Cellular Tissue of do.		100·31	
41. Lungs		101·30	
42. Abdomen		101·30	

FIFTH SET OF EXPERIMENTS.

Made with two Thermo electric Multipliers*

Parts examined.	*Temp. Fahr.*	*Observation.*
XVI. Dog, Female Poodle.	°	
43. Muscle of the Thigh	100·85	
44. Stomach	101·93	
45. Brain	100·85	
The skull was perforated with the trepan in two places, in order that the two ends of the probe might pass out.		The temperature fell, suddenly, several degrees; and in a few minutes after, the animal died.

From the experiments tabulated above, the following conclusions may be drawn:—

1st. There exists a remarkable difference between the temperature of the muscles, and of the cellular tissue, both in man and animals, and which appears to depend,—on the external temperature,—on the manner in which the individual is clothed or covered, and on several other causes. This difference varies from 4°.05 to 2°.25 Fahr. in favour of the muscles. Living bodies are, therefore, similar to inert ones, in cases when their temperature after having been raised is exposed to continued refrigeration in consequence of the medium in which they have been placed. This refrigeration is first evident on the surface, and then proceeds through the interior strata to the centre, according to laws which mathematical analysis has ascertained. But in what manner these successive losses of heat are insensibly restored in man and animals, is not yet known; there is, however, a probability that the method of experimenting now described may enlighten physiology on this point.

2nd. The mean temperature of the muscles of two young men was ascertained to be about 98°.19 Fahr. Let us compare this result with the numbers which have been adopted by several philosophers and physiologists as the mean temperature of the human body.

J. Davy	Temp. of the Human Body	98·00
Desprez	Mean do. of nine Men, aged 30	98·85
Do.	Do. four do. 68	98·83
Do.	Do. four Youths 18	98·55
Hunter	Temp. of the Rectum of a Man in good health from 96°.80 to	98.00

The result given in the tables above is nearly the mean of the numbers found by J. Davy and Desprez. But the observations of these philosophers were made with the thermometer, and, as has been already remarked, it should be recollected that the utility of this instrument is *very* limited, and that it does not indicate instantaneously the temperature *of the medium* into which it may be plunged.

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* *The manner of using two multipliers is not described by the authors.*—Ed.

3rd. The mean temperature of the muscles of several dogs was found to be 100°.94. M. Desprez assigns to the same animal a temperature of 103°.8; a difference of nearly three degrees. MM. Becquerel and Breschet state, that during numerous experiments they never met with so high a temperature. It is probable that this difference may be owing to accidental causes, which M. Desprez could not notice; for it should be remarked that the temperature of the muscles is varied very sensibly by the state of health, and by many other exciting causes. It is this which may furnish an explanation of the minute variations which are frequently observed in results obtained from the same individual, in two different experiments.

4th. In the healthy dog, the temperature of the stomach, of the abdomen, and of the brain, is evidently the same and equal to that of the muscles. One fact worthy of observation, and recorded in experiment 45, is, that the apparatus having indicated 100°.85 for the temperature of the brain, this suddenly fell several degrees, and in a few minutes afterwards the animal ceased to exist.

5th. The experiments 28, 29, with the common carp (*Cyprinus carpio*), gave only about 1° Fahr. between the temperature of its body and that of the water, in favour of the carp.

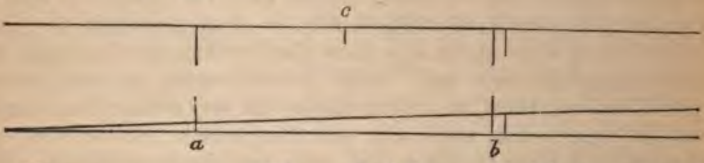
The temperature of the muscles, as before observed, is subject to change from several physical causes. Among the principal are those of contraction, motion, and compression. If a soldered-joint is kept at a constant temperature of about 97° Fahr., and another be placed in the biceps muscle of the arm, on the arm being extended, the needle will deviate about 10°; if the fore-arm is moved about so as to contract the muscle, the deviation will be immediately increased 1 or 2 degrees; then after waiting till the oscillation has ceased, and the needle at rest, if the fore-arm be again moved in the same manner, a new impulse will be given to the needle. Proceeding in this manner, a deviation of 15° may at length be obtained, giving an increase of 5° beyond the primitive deviation, which would correspond to an increase of nearly 1° Fahr. This experiment, which has been repeated a great number of times, proves that contractions of a muscle have the property of increasing its temperature. In order to observe this effect, the apparatus should be able to indicate fifths of a Fahr-degree.

If one of the soldered-joints be held in the biceps muscle, and if with the corresponding arm, wood be sawn for about five minutes, the temperature will rise very sensibly, frequently to 2° Fahr. Agitation, motion, and in general, everything which increases the circulation, tends to elevate the muscular temperature. But is this the only cause? does not the nervous system play its part also? these questions may furnish matter for subsequent communications.

The compression of an artery, on the contrary, diminishes the temperature of the muscles situated beyond the adjacent vessel. If the soldered-joint be inserted in the biceps muscle, or better still in the muscle of the fore-arm, and the humeral artery be strongly compressed by the hand, the motion of the needle will immediately point out a depression of temperature amounting to some fifths of a degree.

not make out: they were probably houses seen through the lower edge of the fog, and refracted also to the upper line. At one spot one of these white objects extended quite across the crystal belt, widening at each end so as to resemble a water-spout. Several fishing-boats were sailing along the coast, their fanciful lateen sails glittering bright in the sun. Suddenly the crystal belt stretched by them, and as suddenly we had their images attached in an inverted position to its upper line: this was the prettiest part of the whole exhibition, the spectre boats being as distinct as the originals, and standing out so clearly from the invisible background. I had scarcely time, however, to direct the attention of some friends to them, when they began to grow indistinct, and in three minutes' time the phantoms could no longer be seen.

At 12 o'clock 30 minutes, the atmosphere began to clear up on both sides of us, and I was lamenting the loss of such an unusual and splendid exhibition, when, towards one o'clock, I found it commencing in the straits of Gibraltar, which had heretofore been the only point of our horizon free from it. I noticed, with a watch in my hand, the different changes which the phenomenon underwent in this place, and the following is a copy of notes taken at the time:



The same appearance of fog, and of the same height, now stretched in a curved line quite across the straits, beginning at Apes'-hill, and ending at Europa Point. At *a*, one-third of the way across, was a merchant brig, with lower and top-gallant studding-sails set: it was steering eastward, and was about twenty-five miles distant from us*: over it was an inverted image of itself, the two tops approaching as near as in the above lines. At *b*, was a two-masted vessel, with its side towards us: its two sets of sails, and also its inverted image above, were very distinct to the naked eye; *a*, and its image were also clearly seen without the glass. This was the state of things at 1 h. 1 m. P.M.

At 1 h. 7 m. the image of *b*, very distinct, but the original is dim. *a*, as before.

At 1 h. 10 m. the image of *b*, easily seen, but the original has entirely disappeared. *a*, as before. With a glass I can now see an inverted vessel at *c*, but none below it.

At 1 h. 13 m. *b*, very distinct above, and the lower vessel again visible. *a*, as before.

At 1 h. 18 m. At *b*, both the lower and upper vessels have disappeared; also the upper one at *a;* and *c* has grown very dim.

At 1 h. 21 m. the inverted vessel above *a*, has not reappeared, but considerably on its right are three inverted vessels, (probably *c*, the image of *b*, and a new image); they are thin and airy-like, but very distinct, and their outline is very well defined; only the images are seen.

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* *I give you the opinion of an old quarter-master as respects the distance, having more confidence in his judgment than my own.*

At 1 h. 23 m. the inverted vessel over *a*, has reappeared, and can be seen with the naked eye. The fog is beginning to grow very thin, and its upper line is becoming indistinct, but the four inverted ships are as distinct as before.

At 1 h. 27 m. the inverted vessel over *a*, has again disappeared: the other three remain as before.

At 1 h. 29 m. the fog has disappeared; the atmosphere resting on the straits being as transparent as any other: still the three inverted ships on the right continue, but they seem now to be suspended in the clear air: they are rather dim. The one over *a*, has not reappeared.

At 1 h. 31 m. those three are still seen, but are quite in the clear sky: the extremities of the fog at Ceuta and the Europa Point still continue: at the latter it is thin; at the former place it is still thick and gray.

At 1 h. 34 m. the three inverted ships still visible, but very dim.

At 1 h. 38 m. they have disappeared, and nothing is now left but the brig at *a*. What has become of the two-masted vessel which, at one o'clock, I saw at *b*? It has not had time to get behind the land, and its masts were then high above the horizon: was this vessel beyond the horizon, and was it thus elevated by refraction? At all events, of the three inverted vessels which four minutes since we saw elevated two hundred feet or more in the sky, the originals cannot now be seen. The horizon is perfectly clear, and I have got the quarter-master to search it carefully with a good glass, but neither of us can see anything but the brig at *b*.

I ought perhaps to add a few words about the weather and winds. On the 12th ult. it commenced blowing from the east, and, on the 18th, increased to a gale, in which all our squadron, except the schooner, broke from one or both their anchors: it lasted ten days, and then became moderate. About the first of this month the wind changed, and for four days we had strong westerly winds: the last two days have been very pleasant, with moderate breezes from the west and south-west; thermometer at the Rock from 68° to 72°. To-day, at 1 o'clock, 13 minutes P.M., the breeze changed suddenly from west to east-north-east, at which point it still continues. I have since ascertained that the Fata Morgana were seen over the straits also during the forenoon. The mountains in Spain, and also some high ridges in Africa, just south of the straits, are covered with snow. The hot winds from the desert crossing these latter, probably become surcharged with vapour, which settles down in the basin between Cape de Gatt and the straits, and thus forms the radius for the reflection which we have just been witnessing. This will account for at least a part of the phenomena.

An officer has just informed me, that four years ago, when lying in the Brandywine, at Algesiras, he saw the same phenomenon around the bay. It lasted about half an hour, and, during this time, the ship's rigging, and the clothing of the persons on deck, were covered with fine cobwebs: the wind was then blowing from the southward, He tells me that he has seen the same while lying in Hampton Roads, Vi[illegible] at a time when the wind had suddenly changed from the north-w[illegible] the *north-eastward*.—(Silliman's *Journal*, No. 60.)

ON ROADS, RAILWAYS, CARRIAGES, AND CANALS.

A Treatise upon Elemental Locomotion, and Interior Communication; by ALEX. GORDON, *Civil Engineer*, &c. 3rd Edit. 1836.

THE circulation of two large editions of this work renders it unnecessary for us to do more than to notice the Supplement which is attached to this new edition. It occupies 20 pages. The subjects of it are a further examination of the alleged superiority of Railroads over Common Roads; and the description of an instrument proposed by the author, for ascertaining and recording the amount of traction, &c., required by any given road. This instrument does not appear to have been yet constructed, and the description being merely verbal, and very general, we are not sure that we discover any striking peculiarity in it. We regret that the author has not carried out his notions into practice, because we fully agree in all that he and others have said on the value of a perfect instrument of the kind; and there is another reason why we think he ought to have done so, at least in diagrams, namely, that he may not be accused of pirating, in a greater or less degree, an instrument which has been for some time constructing by the Messrs. Bramah, for his friend Mr. Macneill, at the order of the Commissioners of Woods and Forests. We suspect no improper motive in Mr. Gordon in the publication of his description, but if he will refer to p. 140 of our March number, he will find an extract of a letter from Mr. Macneill, in which the powers of his new instrument are described, and this remarkable statement:—"*The spring will act in a different way*, its vibrations will not be checked by a piston passing through a fluid, and they will be all registered*"!

We shall say no more on this part of the supplement, except to observe, that with the accomplishments of draughtsman and lithographer, which Mr. Gordon possesses, it would cost him but very little to give that publicity to his design, which Mr. Macneill has promised for the instrument upon which he is engaged, and by this means remove the doubts which so naturally arise in such cases of coincidence.

The other subject of the supplement is, regarding him as a Civil Engineer, peculiarly Mr. Gordon's own. We believe that he now stands, professionally, alone, as the uncompromising advocate of the usual carriage-roads (when made as they ought to be) *versus* railroads, even of the best construction. One or two reputations, of great eminence in the engineering world, had, till very lately, escaped the railroad-contagion, but the genius of that system has, in the present session of parliament, seduced them; he has enlisted them into his ranks, and names that were quoted as familiarly as "Cocker," when the merits of a canal or a road were impeached, and a professional defender thought necessary, are now displayed, not ignominously, perhaps, but certainly triumphantly, by the projectors, at the bottom of prospectuses headed "RAILROAD FROM —— TO ——," as "Acting Engineers," "Consulting Engineers," &c., &c.

Fallen, fallen, fallen,
From their high estate,

in the opinion of Mr. Gordon, and all existing canal-companies.

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* *That is, from a former one made by Mr. Macneill, and purchased by the Prussian government.*

It is certain that in this, as in all questions involving great amounts of property, &c., the statements of partisans should be carefully examined and sifted. This is not an act of great virtue or merit, so long as the questions are subjects of mere contemplation; but when numberless individual interests, great risk of capital, &c., are actually mixed up with them, it requires no common fortitude to take a stand, and maintain it against torrents of popular prejudice, the attacks of deeply-interested persons, and the desertion of estimable and intelligent colleagues. It is this singular position that Mr. Gordon now occupies with regard to the railroad rage. He took it early while others thought with him, and he maintains it now, when he is but "one, and alone"! Though he may call, unheard by the thousands who are busy, scheming, and sharing, and transferring,—by the hundreds of thousands who are watching the hourly issue of new railroad propositions, and the daily fluctuations of railroad-shares, we think there is quite enough in Mr. Gordon's statements and arguments, to demand dispassionate examination by the statesman, the political economist, the great carriers of merchandise and passengers, capitalists disposed to speculate, and, in a more remote degree, the public generally.

Mr. Gordon believes that he has demonstrated the truth of all of the following propositions:—"That the first assertions of railway-engineers, as to the limited velocity of canal-conveyance, has been upset;—that, according to the immutable laws of motion, the edge-railway can merely reduce *surface* resistance;—that all the reduction of surface-resistance which can be effected by an edge-railway is not worth making, when the line is anywhere but on a dead level;—that surface-resistance can be reduced by other means;—that these other means would be more economical, permit of more traffic, and, consequently, furnish more toll-returns to the proprietors;—that a system dangerous to the interests of the public, from its withdrawing repair from existing roads, from its being that of a monopoly, and unavoidably attended with loss of life, could be obviated;—that the velocity of railway-travelling may be attained by other means,—means approved of by several of the most eminent engineers;—that agriculturists might have conveyance much more for their interests than railways, from their exclusive nature, ever can be, and—that the forty millions of pounds proposed to be spent on railways, would prevent the supply of necessary funds for more approved systems of intercourse."

These propositions are of immense importance; and Mr. Gordon has evidently taken very considerable pains to acquire information, both on the road and in the closet, concerning them. The facts he has accumulated he lays before his readers, accompanied by reference to his authorities, and his own conclusions. Up to the present hour he admits of no change in his opinions; and almost the last sentence of his supplement is the repetition of his former affirmation, "that a short time will see the general *edge* railway-system deprecated, as commercially, agriculturally, and politically hurtful." If this prove to be true, what prodigious and expensive mischief is ignorance or knavery spreading at the present moment, over countries that are considered the foremost in science and intelligence!

QUESTIONS FOR SOLUTION RELATING TO METEOROLOGY, HYDROGRAPHY, AND THE ART OF NAVIGATION*.

By M. Arago.

I have somewhere read, that an individual was once lamenting, in presence of D'Alembert, that the Encyclopædia had acquired such a vast extent. You would have had much more reason for complaint, replied the philosopher, if we had drawn up a *negative* Encyclopædia (meaning thereby an Encyclopædia containing a mere indication of things, with which we are unacquainted); for in that case a hundred folio volumes would not have been sufficient.

This reply, I must admit, has hitherto appeared to me to have more point than justice. It is true that the progress of human knowledge shows us daily how far our predecessors were ignorant, and how far we in our turn will appear so to those coming after us; but the greater number of important discoveries have taken place spontaneously, without having been foreseen or suspected by any one. Thus, to cite only two or three examples, D'Alembert's *negative* Encyclopædia could not have contained the most remote allusion to that important and prolific branch of modern physics, now known under the name of Galvanism, or, as it is more properly called, *Voltaic Electricity*. The multiplicity of phenomena, likewise, which are produced by the *polarization of light*, when viewed in relation to its reflection, its ordinary refraction as well as that depending on the action of crystallized plates, would not even be indicated; and the same thing may be said of the theory of *luminous interferences*, in which the singularity of the results is not less remarkable than their infinite variety.

It must be admitted, however, that apart from those important and rare discoveries which are made from time to time all of a sudden, or at least without any visible preparation, and give a new aspect to certain departments of science, there exist important and well-defined questions, which may be confidently recommended to the notice of observers. Having been recently called by the Academy, to draw up instructions regarding physical phenomena, with a view of being transmitted to the Commander of *La Bonite*, I soon perceived that the author of a negative Encyclopædia, even when confining himself to what is distinct and definite, would have to indicate an infinitely greater number of blanks than I was at first inclined to believe. It likewise appeared to me that published notices in relation to these were calculated to be of great utility, and that numerous well-informed persons having their time at their disposal, would receive from them an impulse which would change them from passive contemplators into active partisans of science. The readers of the present work are now therefore acquainted with the reasons which have led me to deviate from the ordinary practice, and substitute in the room of some complete theory in astronomy, physics, or mechanics, an article in which almost everything remains to be solved, since it relates either to what we know imperfectly, or to what we are entirely ignorant of. It will remain for them to decide whether questions so drawn up will lead to the advantages I ascribe to them, or whether the trial should be confined to this first attempt. It is right, however, to inform them that the various questions successively proposed were originally, at least the greater part of them, designed for the officers of a ship (*La Bonite*), commissioned to convey consular agents to Chili, Peru, and the Philippines; I may add, that it was intended that the circumnavigation of this vessel should commence by the way of Cape Horn, and terminate by that of the Cape of Good Hope.

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* These Questions are part of the instructions mentioned at p. 68 of our present *volume*, and we are indebted for them to Jameson's Philosophical *Journal*, No. XL., *1836.*

METEOROLOGICAL PHENOMENA.—In meteorology it is requisite to submit to making observations, which, at the time, are attended with no important result. It is necessary to take care to provide for our successors terms of comparison which we ourselves want, and prepare for them the means of resolving a multitude of important questions, on which it is not competent for us to enter, because the ancients possessed neither barometer nor thermometer. These considerations will suffice to explain our reason for requesting, that, *during the whole voyage of La Bonite*, note should be taken, *both by day and night, and from hour to hour*, of the temperature of the air, of the temperature of the surface of the sea, and of the atmospheric pressure. They will likewise authorize us to hope that these observations will continue to be made with the same zeal, of which an example has been given by the officers of *L'Uranie*, *La Coquille*, *L'Astrolabe*, *La Chevrette*, and *Le Loiret*. At the same time, if unforeseen circumstances require the omission of part of this labour, it would be desirable that the sacrifice should first be made of what is least essential. The details upon which we are about to enter, seem to us calculated, in such cases, to guide the selection to be made by the commander of the expedition.

OBSERVATIONS DESIGNED TO CHARACTERIZE THE PRESENT STATE OF THE GLOBE IN REGARD TO TEMPERATURE.—Has the earth arrived at a permanent state with respect to temperature? The solution of this important question seems to require only the direct comparison of the mean temperatures of the same place, taken at two distant periods. But when we take into account the effects produced by local circumstances, when we consider to what an extent the neighbourhood of a lake, of a forest, of a naked or wooded mountain, of a sandy plain, or one formed of meadows, may modify the temperature, every one will perceive that such thermometrical data alone will not be sufficient; that it is necessary, besides, to ascertain that between the periods in question the country, and even the districts adjoining it, have undergone no important change in their physical aspect and in the nature of their cultivation. It is thus seen that the question becomes singularly complicated, and although numerals are adduced, with sufficient precision to admit of a definite estimate, they become mingled with vague suspicions, which continually throw a scrupulous mind into a state of suspense.

Is there, then, no means of solving the difficulty? These means exist, and are by no means of a complicated nature, for we have only to observe the temperature *in the open sea at a great distance from continents*. If, for this purpose, we make choice of the equinoctial regions, it is not necessary that the observations should be continued for a series of years; the maxima temperatures observed in crossing the line on two or three occasions will be quite sufficient. In the Atlantic, the extremes of these temperatures, as hitherto determined by numerous navigators, are 80°.6 and 84°.2 of Fahr. Taking into account errors in graduation, every one will perceive, that, with a good instrument, the uncertainty of a single observation of the maximum of temperature in the equatorial parts of the Atlantic Ocean, cannot much surpass a degree and that the constancy of the mean of four distinct determinations may be relied on to a small fraction of a degree. Here, then, is a result easy to be obtained, directly connected with the calorific influences on which the temperature of the earth depends, and as much separated as possible from the effects of local circumstances. It ought to form a meteorological gift, which every age should be anxious to bequeath to that which succeeds it. The officers of *La Bonite* will certainly not neglect this part of their instructions. The excellent instruments with which they are furnished, warrants us to expect all that accuracy and precision which the present state of science demands.

OF THE CALORIFIC ACTION OF THE SOLAR RAYS VIEWED IN THEIR RELATION TO THE SITUATION OF PLACES ON THE GLOBE.—Animated discussions have taken place among meteorologists regarding the calorific effects which the solar rays may produce by means of absorption in different countries. Some adduce the observations that have been made towards the arctic circle, from which this singular consequence seems to result, *that the sun has a more powerful heat in high than in low latitudes.* Others refuse to admit this result, on the pretence that it is not proved. The observations made at the equator do not appear to them sufficiently numerous to be taken as one of the terms of comparison; and it is thought, besides, that these observations were made under unfavourable circumstances. This investigation might therefore be recommended to the officers of *La Bonite*. To execute it successfully they would have need of two thermometers, the reservoirs of which, on the one hand, absorb the solar rays unequally, and, on the other, are not too sensible to the cooling influences of currents of air. This double condition may easily be obtained, if, after having procured two thermometers in every respect alike, the bulb of one of them be covered to a certain thickness with white wool, and that of the other with an equal quantity of black wool. These two instruments, exposed to the sun, side by side, will never indicate the same degree; that with the black covering will mount highest. The question, therefore, will consist in determining if the difference of the two indications is less at the equator than at Cape Horn, or at any other higher latitude *.

It will be easily understood that comparative observations of this nature ought to be made at equal altitudes of the sun, and during the most serene weather. Slight differences of altitude, however, will not always impair the accuracy of the observations, if care be taken, under different latitudes, to determine according to what progression the difference of the two instruments increases from sun-rise till mid-day, and diminishes from the latter period till sun-set. Days on which the wind is very high ought to be altogether excluded, whatever be the state of the atmosphere in other respects.

Another observation, somewhat analogous to that of the two thermometers differently covered, will consist in determining the maximum temperature which the sun imparts to a dry soil in equinoctial countries. At Paris, in August 1826, during a serene state of the sky, we found that a thermometer lying horizontally, and having its bulb covered with one millimetre of very fine vegetable mould, stood 129°.2 Fahr. The same instrument, covered to double that depth with river-sand, indicated only 114°.8 Fahr.

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* There are other means still more exact for resolving the problem to which the calorific action of the solar rays has given rise; but these depend on instruments which were not to be found in the hands of our artists at the time of the departure of *La Bonite,* and therefore are not alluded to in the instructions of the Academy. *We will return* to the consideration of them on another opportunity.

EXPERIMENTS TO BE MADE ON THE RADIATION OF THE SKY.—The experiments which we are about to propose ought to give, all other things being equal, the degree of the atmosphere's transparency. This transparency may be appreciated in a manner in some sort inverse and not less interesting, by observations on nocturnal radiation, which are likewise recommended to the commander of *La Bonite*.

It has been known for half a century, that a thermometer placed under a clear sky, on the grass of a meadow, indicates 11°, 12½°, or even 14° Fahr. less than a thermometer, in every respect similar, suspended in the air, at a few feet from the ground. But it is only a few years since an explanation of this phenomenon was given; for it was only in 1817 that Wells established the fact by means of important experiments, and in a thousand different ways, that this inequality of temperature is caused by *the feeble radiating power of a clear sky*.

A screen placed between certain solid bodies and the sky prevents them from cooling, because the screen intercepts their radiating communications with the colder regions of the atmosphere. The clouds act in the same manner; they take the place of the screen. But if we distinguish every vapour which intercepts the solar rays coming from above, or the calorific rays ascending from the earth towards the sky, by the name of a cloud, it cannot be said that the atmosphere is ever entirely free from them. The only difference is their greater or less density.

These differences, however slight they may be, may be indicated by the degree of cold to which solid bodies are reduced in the night; and this accompanying peculiarity is worthy of observation, that the transparency measured in this manner, is the *mean transparency* of the entire firmament, and not that alone of the circumscribed region which may be occupied by a single star.

In order to make these experiments under the most favourable conditions, it is obvious that we must choose bodies which cool most by radiation. According to the researches of Wells, swan-down is the substance that ought to be selected. A thermometer, having its bulb surrounded with this down, should be placed on a table of painted wood supported by slender feet, in a situation where nothing intercepts the view to the horizon. A second thermometer, with the bulb naked, should be suspended in the air at some height above the ground. With regard to the latter, a screen will secure it from all radiation towards the sky. In England, Wells obtained a difference of 15° Fahrenheit between the indications of two thermometers placed in the manner described. It would certainly be strange, if less important differences were to result from them in equinoctial countries, which have been so much praised for the purity of their atmosphere. It is doubtless unnecessary for us to demonstrate the utility that would attach to such experiments, if they were repeated on a very high mountain, such as Mowna-Roa, or Mowna-Kaah, in the Sandwich Islands.

EXAMINATION OF AN ANOMALY WHICH ATMOSPHERIC TEMPERATURES, TAKEN AT DIFFERENT ELEVATIONS, PRESENT IN THE NIGHT, WHEN THE SKY IS CALM AND CLEAR.—The temperature of atmospheric strata diminishes in proportion as these strata become more elevated. There is only one exception to this rule, and that is observed in the night during a calm and clear state of the air. In these circumstances, an increasing progression [illegible] place, to [illegible] certain height. According to the experiments of Pictet, to whom we owe the discovery of this anomaly, a thermometer then suspended in the air at two yards from the ground may indicate throughout the night from 3½° to 5½° Fahr. *less* than a thermometer similarly suspended in the air, but fifteen or sixteen yards higher.

If it be recollected that solid bodies placed on the surface of the ground, pass by means of radiation under a clear sky, to a temperature much below that of the surrounding air, it will not be denied that this air must at length be affected, by means of contact, with the same coldness, and in a greater degree, according as it is nearer the earth. In this, therefore, we find a plausible explanation of the curious fact made known by the natural philosopher of Geneva. Our navigators will impart to it the character of a demonstration, if they repeat Pictet's experiment in the open sea, by comparing, during a clear and tranquil night, a thermometer placed on the deck with another attached to the mast-head. Not that the superficial stratum of the ocean does not experience the same effects of nocturnal radiation, in the same manner as down, wool, grass, &c.; but after its temperature has diminished, this bed of stratum is precipitated, because its specific density has become greater than that of the inferior liquid beds. We are not, therefore, to expect in this case, the enormous local colds observed by Wells in certain bodies placed on the surface of the earth, nor the anomalous coldness of the inferior air, which seems to be the consequence of them. Everything, indeed, leads to the belief, that the increasing progression of atmospheric temperature noticed on land, does not exist in the open sea; and that there the thermometer on the deck, and that at the summit of the mast, will indicate very nearly the same degree. The experiment, nevertheless, is not the less deserving of attention. In the estimation of a prudent natural philosopher, there is always an immense distance between the result of a conjecture and that of an observation.

Expeditious Method of determining Mean Temperatures in Equinoctial Countries.—In our climates, the stratum of the earth which undergoes neither diurnal nor annual variations of temperature, is situated at a great distance from the surface of the ground. But such is not the case in equinoctial regions; for, according to the observations of M. Boussingault, nothing more is necessary than merely to sink a thermometer to the depth of about one foot English, in order to make it indicate constantly the same degree, or very nearly so. Travellers, therefore, may determine very exactly the mean temperature of all the places they visit between the tropics, either in plains or in mountains, by having the precaution to furnish themselves with a miner's piercer, with which it is easy, in a few minutes, to pierce a hole in the ground of the required depth. It will be found that the action of this instrument on rocks and on the soil, occasions a developement of heat, and the observer should always wait till that be entirely dissipated before he commence his experiments. It is likewise necessary that the air in the hole should not be renewed during the whole time of their continuance. A soft substance, such as pasteboard, covered with a large stone, will form a sufficient preventive. The thermometer ought to have a string attached to it, by means of which it may again be drawn up.

The observations of M. Boussingault, of which we have availed ourselves, in order to recommend perforations to the trifling depth of a foot, as conducting very expeditiously to the determination of mean temperatures in all intertropical countries, have been made in sheltered places, in the ground, under Indian huts, and under mere sheds. In these situations, the soil was sheltered from the direct warmth produced by absorption of the solar light, from nocturnal radiation, and infiltration of rains. Every one trying the experiment should place himself in similar circumstances, for there can be no doubt that in the open air, and in places remote from shelter, it would be necessary to penetrate to a much greater depth in the ground, in order to reach the bed possessing an equal temperature.

It is well known that the temperature of the water in wells of moderate depth, also affords an easy and exact mode of ascertaining the mean temperature of the surface. This method, therefore, must not be omitted among those recommended by the Academy.

OBSERVATIONS TO BE MADE ON THERMAL SPRINGS.—If it be the case, as everything leads us to believe, that the high temperatures of the springs called *thermal*, are solely the consequences of the depth from which they rise, it is natural to suppose that the warmest springs should be the least numerous. At the same time, is it not extraordinary, that none have hitherto been observed whose temperature has approached the boiling point within 36° Fahr.[*]? If we are not deceived by some vague reports, the Philippine Islands, that of Luçon in particular, are likely to afford the means of elucidating this subject. There especially, as in many other places where thermal springs exist, the most interesting data that can be collected, are such as tend to prove that the temperature of a very abundant spring varies, or does not vary, with the lapse of ages; and in particular local observations, with a view to show the *necessity* of the fluid having a passage across the very deep-lying strata of the earth.

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[*] We do not include in this category of thermal springs the Geysers of Iceland, and other analogous phenomeaa, which evidently depend on volcanoes at present in a state of activity. The warmest thermal spring, properly so called, with which we are acquainted, *Chaudes Aigues* in Auvergne, is 176° Fahrenheit. Since this article was written for the expedition of *La Bonite*, MM. von Humboldt and Boussingault have given me, as the temperature of the spring *Las Trincheras* (Venézuela) in 1800, 195° Fahr., and in 1823, 206° Fahr. This spring, according to them, has no direct connexion with any active volcano. On the other hand, the Duke of Ragusa writes me, that, at Broussa, at the foot of the Mount Olympus, he found the thermal bath, called by the Turks *Chirurchiest*, to be 183°·2 Fahr. It seems, therefore, that 176° Fahr. is the maximum temperature of European springs only.

[*To be continued.*]

MISCELLANEOUS INTELLIGENCE.

Precision in Scientific Terms. No. II.

Horizontal-Level.—"In common language, the term *level* is the same as *horizontal*; and this may be taken as its true meaning when a small extent of surface is spoken of; but it will be seen to have a very different meaning when applied to the surfaces of fluids of great extent, as the sea, or large lakes.

The earth's figure may be considered as spherical, for the slight deviation from this form may be disregarded in the present inquiry. Now the inequalities on the surface of the terrestrial part of the earth, insignificant though they be when compared with the whole mass of the earth, do not exist at all on the surface of still water, as on the surface of a calm sea. But it is known by observation that the surface of the sea is uniformly curved, and would, if continued uninterruptedly in every direction, present a spherical surface. This is the observed fact, and the same conclusion may be arrived at in the following manner:

Gravity at the earth's surface is a uniform force, and acts in a direction perpendicular to that surface; that is, in the direction of the plumb-line. It acts equally on all bodies at equal distances from the centre of the earth, and unequally on bodies at different distances, exerting a greater action on those at a less distance than on those at a greater. Hence, if the surface of still water have all its points at the same distance from the centre of the earth, every particle at its surface is equally acted on by gravity. But if any two points on its surface be at different distances from the centre, the particles at the points will be acted on unequally; and the pressure at different points of the surface being unequal, the fluid will not be at rest. Hence it is evident, that under the action of a force, such as gravity is known to be, a fluid mass must settle down into a spherical form, and the fluid will not be at rest until it is in this form; and this agrees with what was stated above, as the observed fact.

This spherical surface is called a level surface; and if the whole globe were covered with water, there would have been but one surface, and therefore but one level. But the fact is, that there are many different surfaces; and though theory shows they all ought to be, and observation shows that they all are, spherical surfaces, yet they are not concentric*, but are at very different distances from the centre.

This is expressed by saying that they are all level surfaces, but not all on the same level; and one level is said to be above or below another level, according as it is at a greater or less distance from the centre of the earth."—Webster, *Principles of Hydrostatics.* 1835.

Ashmolean Society—(Feb. 1836.)

The number of Members of the Ashmolean Society of Oxford was, in February last:—

Ordinary Members, 212; Honorary Members, 11; Total, 223.

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* This cannot be true, it is clear from the previous reasoning that they can have *but one* common centre,—that of the earth; and, therefore, must be *concentric.* The *author, by an* inconceivable inadvertency for so accomplished a teacher, seems to have *confounded concentric* with *equidistant.*—Ed.

Prize Subjects, 1836. *Institute of British Architects, London.*

Medals are offered by the Institute of British Architects, for Essays on the following subjects:—

1. "On the Practical Application of the Theory of Sound in the Construction of Edifices, by which the Rules may be ascertained for building Theatres, Churches, Halls, and other places for Public Meetings, in the manner most favourable for the transmission of Sound."

2. "On the Effect which would result to Architecture, in regard to Design and Arrangement, from the general Introduction of Iron in the Construction of Buildings."

The Essays are to be delivered to the Institute on or before the noon of December 31st, of the present year. The Conditions, &c., may be obtained from the honorary secretaries at 43, King Street, Covent Garden.

New mode of preserving Animal Substances.

"The author of this discovery, Sig. Girolamo Segato, is already favourably known to the scientific world, as the author and engraver of improved maps of Africa and Morocco. Ardent in the pursuit of science, he traversed the deserts of Northern Africa, and by his researches, corrected and considerably advanced the knowledge of those regions. It was while travelling in these parts that he received the first hint of this great discovery. In the path of one of those interesting phenomena of the African deserts—a vortex of sand, which his curiosity prompted him to trace, he, one day, discovered a carbonized substance, that upon closer investigation proved to have been originally animal matter, and to have been carbonized by the scorching heat of the sand. He afterwards discovered an entire human carcass, partly black, partly of a sooty hue, about a third less than the ordinary size of man, and all perfectly carbonized. It occurred to him that this accidental process of nature might be imitated by art to the perfect preservation of animal substances. To discover *how* occupied now his whole attention. At the end of some months devoted to this pursuit, the happy thought flashed upon his mind, which was to lead him to the discovery of the desired secret. Compelled to return to Italy by a dangerous malady, brought on by nearly a week's exposure to an unwholesome atmosphere in a pyramid of Abu-Sir, which he had entered for the purpose of extending his scientific researches, he was obliged to intermit for a time his favourite pursuit, but, after regaining his health, he again gave himself to it with renewed ardour; and, after a short time, succeeded to the highest degree of his most sanguine expectations.

The following are some of the results obtained by the discovery. Entire animal bodies yield as readily to the process as small portions. They become hard, taking a consistence entirely stony. The skin, muscles, nerves, veins, blood, &c., all undergo this wonderful change; and to effect this, it is not necessary to remove any part of the viscera. The colour, forms, and general characters of the parts remain the same. Offensive substances lose their smell. Putrefaction is checked at once. What is most wonderful of all is, that if the process be carried only to a given degree, the joints remain perfectly flexible. Skeletons even remain united by their own natural ligaments, which become solid although they retain their pliancy. Moisture and insects never injure them. Their volume diminishes a little; their weight remains almost the same. Hair continues firm in its place, and retains its natural appearance. Birds and fishes lose neither their feathers, membranes, scales, nor colours. The insect preserves its minutest appendage. The eyes in most animals sparkle as in life, and from their want of motion alone would you suppose vitality extinct.

The following are some of the objects that have been subjected to the petrifying process, and are now exhibited in the *studio* of Sig. Segato. One of the first of his experiments was performed upon a canary-bird (*Fringilla canaria*, Lin.) It is still preserved unaltered, although it is now ten years since the experiment was performed; and it has been submitted to the action of water and of insects. A parrot (*Psittacus æstivus*, Lin.) retains its original brilliancy of plumage unimpaired. Eggs of the land-turtle, turtles, various tarantulæ, a water-snake, a toad, various kinds of fish, snails, and insects, are in a perfect state of preservation. To these are added various parts of the human body. A hand of a lady, who died of consumption, preserves the emaciation of the disease and of death. Another of a man is flexible in the different phalangic articulations, and yet unalterable; a foot with the nails perfectly fast; a collection of all the intestines of a child, in their natural colours and forms, with the fecal matters unremoved; the liver of a man who died from intemperance, dark and lustrous like ebony; an entire human brain with its convolutions, of extreme hardness; the skin of a woman's breast naturally configured; a pate of a girl, perfectly flexible, from which the hair hangs in curls; the head of an infant, partly destroyed and discoloured by putrefaction. There is also in the cabinet of Sig. Segato, a table constructed as follows: a spheroidal surface of wood contains a parallelogram, composed of 214 pieces, regularly arranged. These to the eye appear like the most beautiful *pietre dure* that have been produced by nature. Their various colours, polish, and splendour, and their surprising hardness, would leave no doubt of their stony character. The sharpest file, with difficulty, makes an impression on any of them, some it does not attack at all. These pieces are all portions of the human body, hardened by this new process; as the heart, liver, pancreas, spleen, tongue, brain, arteries, &c., &c., all resembling the most highly polished precious marbles. An entire body has not yet been tried, principally on account of the limited resources of Sig. Segato, although the expense would be about one-tenth only of that of embalming by the ordinary process.

Great advantages to science, especially to natural history and human anatomy, are expected to result from this discovery; and it is even confidently believed, that the remains of friends, of men of science and of worth, may be preserved for ages in the exact form and appearance in which the hand of death found them, with nothing offensive or repulsive about them.

As vouchers for the accuracy of the statements, the certificates of many of the most distinguished physicians, professors, and men of science in Florence, where Sig. Segato resides, are appended. Among them it is sufficient to mention the names of Sig. Betti, professor of Physiology; Sig. Zannetti, professor of Human Anatomy; and Dr. Gazzeri, professor of Chemistry. Day, *From a pamphlet published in Florence in the Summer of* 1835.—Silliman's *Journal*, No. LX., 1836.

Remarkable Sealing-Wax used by the Grand Vizier.

DURING an audience of the British Ambassador, attended by his suite, at the Turkish court, it was observed that "the vizier sent a sealed paper wrapped in muslin, by a messenger, to the sultan. In sealing this letter with red wax, he used no candle, or any other process that I could see, to dissolve it, so as to *make it* susceptible of an impression, though he impressed a seal upon it."—*Walsh, Residence at Constantinople*. London, 1836.

Railroad between Brussels and Antwerp.

THE execution of the Railroad between Brussels and Antwerp was divided into two sections: the first reaching from Brussels to Malines, a distance, by the rails, of 13½ English miles; and the second, from Malines to Antwerp, 14½ miles. The Brussels and Malines Line has been completed, and traversed regularly by carriages for some time. The Malines and Antwerp Line has been recently completed, and on the 4th ult. was opened at Antwerp, in the presence of the king, queen, and court of Belgium, with great ceremony. The design, proposed advantages, and important consequences, of the undertaking, were pointed out in an address to the king, delivered by M. Rogier, the governor of Antwerp, to whom Belgium is principally indebted for the first impulse on the subject. He described it as the means by which Belgium is to be rendered by land what England and Holland are by sea,—a carrier of general commerce. "Antwerp is now but an hour from Brussels; the Railroad is a link of that immense chain which, in a little time, will connect every state. In six or seven hours we may then enter Paris; in sixteen, Berlin; in sixty, Petersburgh. Upon a railroad, the tour of the world might be made in six weeks."

The country through which this Belgian line runs, presents, as might be expected, none of the usual causes of extra expense; no extensive viaducts, large bridges, or tunnels, were necessary. The clivities were extremely moderate; on the Brussels section, the greatest difference of level between any two points of the road is but thirty-two feet; on the Antwerp section, thirty-six feet. The Engineers were Messrs. Simmons and De Ridders, whose talents and exertions are spoken of as eminent and successful. The total cost of the whole line, exclusive of locomoteurs and wagons, was 3,200,000 fr. =128,000*l*., or about 4500*l*. per mile.

Six journeys per day are now made from Brussels to Antwerp, and *vice versâ*, between half-past 6 A.M. and half-past 6 P.M. The trains start simultaneously at Brussels and Antwerp, and meet at Malines. The whole length, including stoppages, is stated to be run in an hour and a half, and the average velocity to be twenty-two miles per hour. There are four kinds of carriages for passengers in each train. The *Berlines* at 3½ francs—about 3*s*.; the *Diligences* at 3 fr.—2*s*. 6*d*.; the *Char-à-bancs* (covered), 2 fr.—1*s*. 10*d*.; and open wagons at 1 fr. 20 cents.—1*s*. 2*d*. This means of reaching Brussels six times a day in an hour and a half, will give Antwerp an important advantage over the other steamer sea-ports. Two of the locomoteurs, the *Stephenson*, and another, were built in England, the other two, *La Belge* and *L'Anversoise*, one of 10, and the other of 20 horse-power, were constructed by Mr. Cockerill, in Belgium, at Seraing.

When the whole system of Railroad, now contemplated in Belgium, is completed, and which is sanguinely looked for by the end of 1839, Malines will be the centre from which four most important Railroads will radiate, *viz*., those of Brussels, Liege, Antwerp, and Ghent. Nothing will then be wanting to crown the undertaking with abundant success but that amelioration of the duties which M. Rogier entreated of the king,—that liberal system of Transit and Bond, by which merchandise, as well as passengers, may traverse the kingdom, from the sea to the Continent, and *vice versâ*, with scarcely any evidence of its having changed elements or *country*.

Observed Effects of the late Eclipse on Atmospheric Temperature and the Heat of the Solar Rays.

THE spot selected by some observers of the eclipse, on the 15th ult., was the summit of Shooter's Hill, about six miles from London, near Woolwich, in Kent: latitude 51° 28′ N., longitude in time 15 sec. E., by the Ordnance survey.

A Thermometer (marked A in the following table) was fully exposed to the sun's rays, before the commencement of the eclipse; another (B) was placed in the shade. Gunpowder was also exposed to the solar rays, concentrated by a lens, and the times observed that were required to inflame it.

The times, observations, &c., are arranged in the following table;—

	Greenwich mean time.				Thermometers.		Gunpowder inflamed in	
	h.	min.	sec.	Stage of the Eclipse	A	B	min.	sec.
1					92°	61°	0	3
2	I.	51	33	Commencement, observed by 2 persons				
3	II.	25	00		82°	63°		
4	—	45	00		72°	60°		
5	—	54	00				0	12
6	III.	7	00		68°	59°		
7	—	15	00				did not inflame	
8	—	15	19	Middle, greatest obscuration				
9	—	16	00		65°	58°		
10	—	25	00		64°, Greatest depression 28°, stationary about 5 minutes	58°		
11	—	33	30		64½°	57½°, Greatest depression 3½°		
12	—	37	00		65°	58°		
13	—	39	00				3	30
14	—	45	00				0	15
15	—	47	00		68°	58°		
16	—	50	00		70°	58½°	0	11
17	—	55	00		73°	59°		
18	IV.	16	00		75°	59°		
19	—	39	05	End, observed by 4 persons	76°	60°	0	03

About the middle of the eclipse, the light assumed a very faint hue, similar to that of very early morning or late evening. The diminution of temperature at this time gave to the human frame a sensation of evening chilliness, to such a degree that the garden-flowers, around the lawn at the place of observation, appeared closing, as if for the night. The poultry commenced retiring as early as six minutes past three, and at the time of greatest obscuration, nine hens and a cock, being the whole of the family, had gone to roost.

Appulses of the moon's limb to several of the numerous spots on the solar disk were also observed.

Additional Remarks on the Eclipse of the Sun on the 15th of May, 1836, by our Meteorological Correspondent, Blackheath-hill, Kent.

THE day very fine, and nearly cloudless; a strong haze, occasioned by the London smoke, to the westward; the barometer about 30.60, and nearly stationary, only the diurnal falling perceptible, viz. about .03. The thermometer at the commencement of the Eclipse,—

In the sun	81°
In the shade	67°

And at the greatest obscuration, *(or a little after,)*

In the shade it had fallen to	62°
In the sun to	66°
And the Radiator on the ground to	59°

The dew-point, by Daniell's Hygrometer, marked twelve at the commencement, and only seven at the time of the greatest darkness, or, to be plainer, the point of deposition was as high as 57°.

The sky, when clear and free from haze,* assumed a much deeper blue, and the gloom was very considerable. The foliage of the trees appeared tinged with orange colour. The planet Venus was seen, about three o'clock, with the naked eye. There was a little wind from the N.E., but just after the middle of the Eclipse it suddenly changed to the S. and S.E., blowing fresh and cold as to sense; after sunset, it gradually got back to N.E. Towards evening I looked out for Venus, and about a quarter of an hour before sunset estimated her brightness to be the same as during the middle of the Eclipse, from which it may be inferred, that daylight was diminished to nearly sunsetting. At the time of the greatest darkness, the birds appeared agitated, and among domestic fowls there were evident signs of alarm, the cocks filling the air with their continued crowing.

A Word or two about the REFRACTOR, *lately put up at the Royal Observatory at Bogenhausen, near Munich.*

[In a Letter from a Correspondent, dated 10th May, 1836.]

"TOWARDS the close of last Autumn the principal arrangements connected with the building destined for the reception of the Great Refractor were completed, the instrument was therefore put up, with all due care, some three months or so since.

But before speaking of the instrument, it may be as well to state that the building in which it stands differs very materially in its construction from any that till now have been built for this purpose. In place of the usual rotatory dome, provided with sliding shutters, it is furnished with a square roof, running so easily on wheels, that a very moderate force, applied by means of a winch, removes it entirely away from the telescope. The instrument is thus, as it were, erected in the open air. This advantage, by which all currents arising from the inequality of internal and external temperature, are naturally done away with, combined with the economy attending its construction, are the chief reasons which brought about the introduction of a plan which (as far at least as the limited experience of a few months allows one to decide) answers its purpose most effectually.

The dimensions of the Refractor, are, in English measure,—Aperture, 11·190 inches; focal length, 15·987 feet.

The highest power which has yet been applied to it is 1200. In a late Number of the *Astronomische Nachrichten*, Professor Struve gives an account of its powers compared with the Dorpat Refractor, the only instrument executed in all its details by Fraunhofer with which a comparison can be instituted. Professor Struve brought with him hither, from Dorpat, the tablets that he employed as test-objects for his Refractor, in order to subject the Bogenhausen instrument to a similar trial. The result of his examination was *not only that the latter* showed all the dots and lines which the former exhibited, but that it gave the more minute details of these objects, with even greater distinctness.

After a careful examination, Professor Struve gives his opinion of the instrument in the following words:—"The Bogenhausen Refractor is conceived and executed in the true spirit of Fraunhofer, inasmuch as it combines the two chief requisites, the greatest distinctness, and a perfectly colourless image."

Indeed, its powers appear to exceed those of Sir J. F. Herschel's Reflector, for the objects which that astronomer cites as difficult to make out, are shown by it with the greatest readiness; for instance, the small double star near β Equulei, or the minute companion of α^2 Cancri. The position and distance of this latter star have long been determined by this Refractor; the Reflector could not do as much; but then it is fair to state that this may be owing to the mirror being "much tarnished."

The Dorpat Refractor was originally furnished with no power above 700; but as, lately, eye-pieces as high as 1000 have been adapted to it with success, it may perhaps be found that the instrument lately erected here will bear a much higher power than that mentioned above as the extent of its present range. Indeed, when it is mentioned that 1200 may, under favourable circumstances, be used for Saturn, it can hardly be doubted that a higher power may be applied to stars. Two object-glasses of similar aperture and focal length, and in every respect equally perfect, were made here at the same time for this Refractor. The remaining one is yet for sale at the Optical Institute, and the price demanded for it is said to be 10,000 florins, that is to say, about 800 guineas. Is it not very desirable it should find its way to England? The Optical Institute has now in hand Refractors of much larger dimensions than this. It will not be long before one is completed of 12·789, and another of, it is said, even 14·921 inches aperture."——Ω.

Bequest to "Ingenious Men and Women."

"JOHN SCOTT, chemist, late of Edinburgh, by his will, made in the year 1816, bequeathed the sum of four thousand dollars in the funded 3 per cent. stock of the United States, to the corporation of the city of Philadelphia, directing that the interest and dividend to become receivable thereon, should be laid out in premiums, to be distributed among ingenious men and women, who make useful inventions, but no such premium to exceed twenty dollars*, and that therewith shall be given a copper medal, with this inscription, 'To the most deserving.'"

Such are the terms in which this liberal Scotchman devoted about £530. for ever, to the extension and improvement of the useful arts.

The patronage which the distribution of money so appropriated would confer, has, by a most honourable act of self-denial on the part of the corporation of the city of Philadelphia, been delegated to the *Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts,* a scientific and meritorious institution, of vigorous growth, in the same city.

We regret to be obliged to remark, that the first move of the Institute in this matter is not conceived, either in the cosmopolitan spirit of the foreigner to their shores who made the bequest, nor in that disinterested sentiment which *actuated* the corporation. The Institute has published the particulars of the *legacy,* and of the conditions which are to regulate the distribution of the premiums, "for the information of the ingenious throughout *the United States!*"

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* About £4. 10s.

This selfishness of patriotism had no dwelling in the heart of the Edinburgh chemist, he drew no petty circle of privilege round the United States, nor round any particular state, kingdom, nor empire, nor even as a "lord of the creation," round his own sex; his remarkable phrase is "TO INGENIOUS MEN AND WOMEN," and we do an act of duty to the memory of this benefactor to the useful arts and to his species, and to the vast scattered family of the ingenious whom he intended to stimulate and reward, by adding a rider to the programme of the Franklin Institute, and sending it out as extensively as our means can accomplish, to announce this bequest "to ingenious men and women," wherever they may exist, or of whatever colour they may be tinted. We shall, to further this extension, adopt a very useful plan, well known and frequently practised among our Trans-Atlantic brethren. We invite the editors of every periodical whatever—magazine, journal, or newspaper, in every country and language, to print gratuitously and display conspicuously, the paragraph containing Mr. Scott's legacy, in order that it may have the greatest possible circulation, and be known among all who may have the right to become candidates. The number of these we estimate at about 740 millions, instead of twelve millions,—the population of the United States; and to be spread over a surface of above 37 millions of geographical square miles, instead of less than two millions, the area to which the Franklin Institute confine their information.

Science assisted by the State. No. II.

We noticed, p. 68 of the present volume, an instance in which some scientific inquiries were facilitated by the French minister, and urged the important services which could be rendered to Science by governments and other public bodies, if they would take advantage of the means within their reach.

We are happy to record another case: in this, the United States' government has distinguished itself, by requesting of the Franklin Institute of Pennsylvania for the Promotion of the Mechanic Arts, the designing and conducting of a series of experiments, the object of which should be "to test the truth or falsity of the various causes assigned for the explosions of Steam-boilers, with a view to the remedies either proposed, or which may be consequent upon the result of the investigation."

A more important inquiry could scarcely be suggested on the present very extensive, and still most rapidly extending, use of Steam-boilers. This request of the United States' government, has, we presume, been accompanied by a suitable grant of funds, as the Institute has proceeded "to provide for the experiments an apparatus of such dimensions, as to furnish results applicable to practice." But we think this important fact about the funds should not have been left to conjecture, in justice to the government, or to the Hon. S. D. Ingham, late Secretary of the Treasury Department, who has the high merit of being the originator of the investigation. It would, also, be instructive to know at what expense (though no sum, properly expended, could be too large), such inquiries can be conducted.

The Institute have completed the investigation of no less than twelve queries relating to the subject, and are publishing their report in successive numbers of their Journal. When this is completed, we shall lay an analysis [illegible] it, at least, before our readers.

Locomoteurs on Common Roads.

"Mr. Goldsworthy Gurney has now contracted with wealthy and influential parties to build Steam-carriages for the road between Plymouth and Devonport; these parties have entered upon the speculation with a view to extend, hereafter, their operations on a much longer line."—Gordon, ***Treatise on Elemental Locomotion.*** May, 1836.

Railroad Acts, present Session, (May 25th incl.)

The following Railroad Bills, in addition to No. 1 (see p. 275), received the royal assent on May 19th;—2. Arbroath and Forfar; 3. Great Western Railway Act Amendment; 4. Birmingham and Derby; 5. Ulster; 6. Dundee and Arbroath; 7. Bristol and Exeter; 8. Aylesbury; and, 9. Bolton and Leigh, on May 20th.

Patent Law Grievance. No. III.

The penalties inflicted on the inventive genius of Britain during the present year, up to the 26th ult., in the shape of government stamps and fees on patents, amount to more than £17,000!

N.B. This sum has been paid in *ready money*, on taking the first steps, and as many of the inventors are poor men (operatives), and a great many others of them persons to whom it would be very inconvenient to pay at least £100. down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

Hope Deferred.

We mentioned, p. 213, that Mr. Mackinnon had given notice in the House of Commons, on the 21st of March, that he should move for a Committee on the amelioration of the Patent Law on the 21st of April; from the 21st of April he deferred it until the 19th of May; from the 19th of May he has deferred it until the 14th of June!

NEW PATENTS. 1836.

ENGLISH.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

GRANTS.

APRIL *contd.*

101. William Preston, Sunnyside, *Lanc.*, Operative Calico-printer; for improvements in printing of calico and other fabrics. Apr. 28.—Oct. 28.

102. John Burns Smith, Salford, *Lanc.*, Cotton-spinner; for improvements in the machinery for roving, spinning, and twisting cotton and other fibrous substances. Apr. 30.—Oct. *30.*

Total, April...23.

MAY.

103. John Whiting, Rodney-buildings, New Kent-road, *Surr.*, M.D.; for improvements in preparing certain farinaceous food. May 3.—Nov. 3.

104. John Macneill, Parliament-st., *Middx.*, Civil-engineer; for improvements in making or mending turnpike or common roads. May 3.—Nov. 3.

105. Henry Sharpe, Broad-st.-buildings, Lond., Merchant; for improvements

in sawing wood and other materials. May 30.—Nov. 3. *For. Comm.*

106. William Sneath, Ison-Green, *Nott.*, Lace-maker; for improvements in machinery, by aid of which, thread-work ornaments of certain kinds can be formed in net or lace made by bobbin-net machinery. May 3.—Nov. 3.

107. William Augustus Howell, Ramsgate, *Kent*, Smith and Ironmonger; for improvements in the construction of springs for doors. May 3.—Nov. 3.

108. Thomas Henry Russell, Took's-court, *Lond.* Tube-maker; for improvements in making welded iron-tubes. May 3.—Nov. 3.

109. Edmund Pontifex, Shoe-lane, *Lond.*, Coppersmith; for an improvement in making and refining sugar. May 5.—Nov. 5. *For. Comm.*

110. Joseph Banister, Colchester, *Essex*, Watch-maker; for improvements in watches and other time-keepers. May 7.—Nov. 7.

111. John Elvey, Canterbury, *Kent*, Millwright; for improvements in steam-engines. May 7.—Nov. 7.

112. Matthew Hawthornthwaite, Kendal, *Westm.*, Weaver; for a new mode of producing certain patterns in certain woven goods. May 7.—Nov. 7.

113. Thomas Taylor, Banbury, *Oxf.*, Saddler and Harness-maker; for improvements in saddles for riding. May 7.—Nov. 7.

114. Luke Hebert, No. 20, Paternoster-row, *Lond.*; for improvements in horse collars. May 9.—Nov. 9. *For. Comm.*

115. John Hague, Cable-st., Wellclose-sq., *Middx.*, Engineer; for an invention for raising water by the application and arrangement of a well-known power from mines, excavations, holds of ships or vessels, and other places where water may be deposited or accumulated whether from accidental or natural causes, and also applying such power to, and in giving motion to, certain machinery. May 9.—July 9.

116. Richard Waddington, and John Hardman, Bradford, *York.*, Iron-founders; for an improved method of making and constructing wheels for railway carriages. May 10.—Nov. 10.

117. Richard Birkin, Basford, *Nott.*, Lace-manufacturer; for improvements in machinery for making lace, commonly called ornamented bobbin-net-lace. May 11.—Nov. 11.

118. Richard Wilson, Blyth Sheds, *Northumb.*, Builder; for improvements in making fire-places, slabs, columns, monuments, and cornices, such as have heretofore been made of marble. May 12.—Nov. 12.

119. Thomas Grahame, of Nantes, France, but now of Suffolk-st., Pall Mall, *Middx.*, Gent.; for improvements in passing boats and other bodies from one level to another. May 13.—Nov. 13.

120. John Ashdowne, Tunbridge, *Kent.*, Gent.; for improvements in apparatus to be added to wheels to facilitate the draft of carriages on turnpike and common roads. May 13.—Nov. 13.

121. Wheatley Kirk, Commercial-st., Leeds, *York*, Music-seller and manufacturer of Piano-fortes, for improvements in piano-fortes. May 14.—Nov. 14.

122. Joseph Whitworth, Manchester, *Lanc.*, Engineer; for improvements in machinery for spinning and doubling cotton wool and other fibrous substances. May 17.—Nov. 17.

123. David Fisher, Wolverhampton, *Staff.*, Mechanic; for an improvement in steam-engines. May 17.—Nov. 17.

124. Henry Walker Wood, No. 29, Austin-friars, *Lond.*, Merchant; for improvements in certain locomotive apparatus. May 17.—Nov. 17.

125. James Brown, Esk Mills, Penny-cuick, *N. B.*, Paper-maker; for improvements in apparatus for making paper. May 18.—Nov. 18.

126. Thomas Beck, Little Stoneham, *Suff.*, Gent.; for improved apparatus for obtaining power and motion to be used as a mechanical agent generally, which he intends to denominate Rotæ Vivæ. May 18.—Nov. 18.

127. Pierre Barthelemy Guinibert Debac, Brixton, *Surr.*, Civil-engineer; for improvements in railways. May 18.—Nov. 18.

128. Henry Elkington, Birmingham, *Warw.*, Gent.; for an improved rotary steam-engine. May 23.—Nov. 23.

129. William Watson, Leeds, *York.*, Dyer; for an improvement in dying hats, by the application of certain chemical matters never before applied to that purpose. May 24.—Nov. 24.

METEOROLOGICAL JOURNAL FOR APRIL, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month	Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer		Daily Temp.	Solar Var.	Rad.	Clouds.		Wind.		Direction of wind		Lunation.	WEATHER, &c.
					Min.	Max.				A.M.	P.M.	A.M.	P.M.	A.M.	P.M.		
Friday, 1	29·940	53°	29·715	53°	35°6	46°0	40·8	10°4	34°	9	10	1	2	S.W.S.	E.N.E.	○	Clouds; *cirro-stratus*; continued rain and snow.
Satur. 2	29·650	49	29·749	50	33·4	43·6	38·5	10·2	32	5	7	3	2	W. b N.	W.N.W.		Showers of sleet and snow; night showery.
Sun. 3	30·055	49	30·225	50	32·0	42·0	37·0	10·0	31	7	8	3	4·3	N. b W.	N.		Windy A.M.; Violent wind with hail-showers; fine
Mon. 4	30·500	49	30·464	51	30·8	46·2	38·5	15·4	29	4	4	1	1	N.	N.		Cold air; drier *cumuli*; clear frosty night. [evening.
Tues. 5	30·326	50	30·196	52	28·6	52·5	40·6	23·9	28	8	10	1	1	S.	S.		*Cirro-stratus*; rain P.M.
Wed. 6	30·041	53	30·000	53	39·5	51·0	45·3	11·5	36	10	9	0	1	S.W.	S.W.		Small rain A.M.; overcast and gloomy. [night.
Thurs. 7	29·512	53	29·304	53	38·7	51·3	45·0	12·6	37	10	6	2	1	S.W.	W.		Rain till two P.M.; large dense *cumuli*; lightning at
Friday, 8	29·149	54	29·245	55	35·0	52·0	43·5	17·0	32	10	5	1	1	S. b W.	S.	☾	Rain A.M.; *nimbi*, with rainbow at three afternoon.
Satur. 9	29·424	55	29·485	55	38·9	49·5	44·2	10·6	37	9	6	1	0	S.E.	S.S.E.		Heavy showers with hail; evening alternately clear
Sun. 10	29·696	55	29·706	55	36·8	53·6	45·2	16·8	35	4	5	2	1	E.	N.E.		*Cumuli*; *nimbi*, with showers. [and cloudy.
Mon. 11	29·854	55	29·852	56	41·8	48·9	45.3	7·1	40	10	8	1	0	N.	N.W.		Thick, hazy, and cloudy; clear night.
Tues. 12	29·896	55	29·904	56	32·8	55·9	44·4	23·1	29	10	9	2	1	W.S.W.	S.W.		Mostly cloudy, light showers.
Wed. 13	29·972	54	29·896	56	44·6	57·5	51·1	12·9	41	9	8	2	3	W.S.W.	W.S.W.		Ditto ditto with wind. [N.E. at midnight.
Thurs. 14	30·128	56	30·130	58	40·2	54·0	47·1	13·8	37	10	10	0	0	W.	W.		Drizzling rain; cloudy throughout; wind changed to
Friday, 15	30·285	57	30·301	59	46·0	56·5	51·3	10·5	45	8	2	1	1	E.	E.	●	Mostly cloudy A.M.; clear night. [night.
Satur. 16	30·301	55	30·216	58	31·9	56·7	44.3	24·8	30	2	10	1	0	N.	N.W.		Thick fog; *stratus* A.M.; *cirro-cum.*; close; cloudy
Sun. 17	30·250	56	30·251	56	41·8	47·2	44·5	5·4	40	10	10	0	1	E.	N.		Thick small rain; very dark and misty.
Mon. 18	30·246	57	30·208	57	38·2	53·9	46·0	15·7	35	7	7	1	1	N.W.	N.		Fair; afternoon and evening dense clouds; *cum.* and
Tues. 19	30·258	58	30·210	59	40·9	56·4	48·7	15·5	38	3	10	1	1	W.	W.		Ditto A.M.; overcast. [*cum-strat.*
Wed. 20	30·145	57	30·022	58	46·8	56·5	51·7	9·7	44	9	8	3	3	S.W.	S.W.		Much cloud; windy.
Thurs. 21	30·071	56	30·046	57	40·9	57·0	49·0	16·1	39	3	8	1	1	W.	W.		Fine A.M.; *cirro-cum.* and *cumulo-stratus.* [at night
Friday, 22	29·912	58	29·986	58	45·8	58·9	52·4	13·1	45	7	3	2	2	S.W.	W.		Rain early in the morn.; fair; bril. aurora borealis
Satur. 23	30·035	59	29·950	59	42·5	54·5	48·5	12·0	40	5	6	2	2	S.W.	S.	☽	Rain till one P.M; afternoon fine; night dark and
Sun. 24	29·850	58	29·895	59	42·6	48·9	45·7	6·3	41	10	10	2	2	E.	E.N.E.		Incessant rain, and very cold. [lowering
Mon. 25	30·246	56	30·250	58	39·0	54·9	46·9	15·9	36	7	2	2	1	N.E.	N.W.		Fine.
Tues. 26	30·242	56	30.112	57	40·9	51·0	46·0	10·1	38	2	9	1	1	N.W.	W.N.W.		Cloudy; air very sharp; squally at night.
Wed. 27	29·921	55	29·931	56	35·6	48·5	42·1	12·9	33	6	5	2	2	N. b E.	N.E.		Cold bleak winds; showers of rain and sleet.
Thurs. 28	30·058	55	29·950	56	35·2	51·6	43·4	16·4	33	2	8	1	2	N.W.	W.N.		Much cloud; hazy; rain about 6 P.M., and low temp.
Friday, 29	30·019	51	29·981	52	33·1	43·9	38·5	10·8	32	4	7	3	2	N.	N.		Snow showers; cutting cold wind; *nimbi.*
Satur. 30	29·902	49	29·852	53	27·8	48·0	37·9	20·2	26	7	6	1	1	N.N.W.	N. b W.		Sharp frost; thin ice at sunrise; *cum.* & *cum.-stratus.*
Mean	29·999	55	29·968	56	37·92	51·61	44·78	13·69									

Bar. Max. 30·500 on the 4th. || Ther. Max. 58°9 on the 22nd. || Lowest point of Rad. 26°, on the 30th.
Bar. Min. 29·149 8th. || Ther. Min. 27°8 30th. || Rain fallen 3·040.

THE

MAGAZINE OF POPULAR SCIENCE,

AND

JOURNAL OF THE USEFUL ARTS.

RECENT RESEARCHES ON LIGHT.

Few branches of science have undergone a more entire renovation of late years, than that which relates to the phenomena presented by the subtile and mysterious agent, light, and the theoretical views by which those phenomena may be explained.

It is a somewhat remarkable feature in the history of physical optics, that the most complicated appearances which experiment exhibits, are, in many instances, among those which are the most perfectly understood, and explained on mathematical principles; whilst some of the simplest kind, and which are everywhere familiar to us, are among the number of those which have long remained without illustration; and, even now, have scarcely received any complete elucidation. There are few parts of the subject in reference to which the above remark has been more strikingly exemplified, than the phenomena and theory of prismatic dispersion.

In attempting a popular sketch of the most important points of recent investigation connected with this curious branch of science, we shall find it necessary to offer a few preliminary illustrations of the nature of prismatic dispersion; especially, as it is a point on which, even among scientific men, very vague and imperfect conceptions have often prevailed.

REFRACTION AND DISPERSION.

The ancients succeeded in perfectly tracing the law and consequences of the *reflection* of light; the law, indeed, was so simple, (viz., that the angle of reflection is always equal to that of incidence,) that to the mathematicians of antiquity, it held out an inviting field for the application of geometrical skill; which was early employed for tracing a variety of theorems resulting from that law, and when the first physical principle was established, the whole of "Catoptrics," was little more than a continued exercise of geometrical deduction from them. Not so with "Dioptrics," or the investigation of the course and properties of *transmitted* light. It was long, in this case, before the very first principle was discovered.

That a ray of light, entering obliquely out of a rare medium, as air, into a dense medium, as glass, or water, bounded by a plane surface, deviates from its previous rectilinear course, and takes a new, but still rectilinear, path within the new medium, was observed as the fundamental fact of dioptrics, long before any theory was imagined, by which it could be accounted for. *What* particular direction it would take under particular circumstances was also a subject of inquiry: and it was early *observed, that if a* perpendicular to the surface be imagined drawn at the point where the ray falls upon it, its course will still continue in the same plane, but its deviation in direction will be of such a kind that it falls *nearer to the perpendicular* than it did before.

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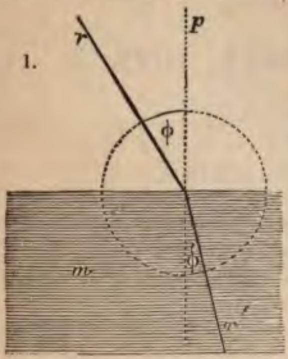

In the annexed sketch the ray r, falls upon the plane surface of the dense medium m, and takes a new course r', *nearer* to the perpendicular p, that is, forming a less angle ϕ' with it, than that which it formed before, Φ. Observers for a long time could not discover anything in the way of a more precise relation, or law, than this. At length, however, upon the comparison of a number of observations, it appeared that the new angle ϕ' (which is called the angle of refraction, as ϕ is called the angle of incidence,) always bears a certain relation in magnitude to ϕ, and that its actual amount varies very considerably in different substances. In any one medium the angles are *not simply proportional* one to the other, but bear a somewhat more complex relation, which is expressed by the trigonometrical law that *their* SINES *are in a constant ratio.* This law was discovered by Snell, (1619,) and is the foundation of optics. The absolute value of the constant ratio is different for different media, and is called "the refractive index" of the medium. When the ray of light arrives at the second surface of the medium, precisely the same thing takes place in reverse order. Thus, if the surfaces be inclined to one another, the ray will undergo a new deviation at the second surface, which may augment its entire deviation from its original course. This will be evident by looking at the course of such a ray traced in the annexed figure 2, where the successive angles are marked $\phi\ \phi'\ \phi''\ \phi'''$ and m is a dense transparent medium surrounded by air. Now this medium m, with inclined surfaces as here represented, obviously forms a portion of a triangular prism, and the deviation which a ray undergoes is thus magnified by its passage through two inclined surfaces, so that by this means we have the best experimental method of measuring the effect of refraction in different media.

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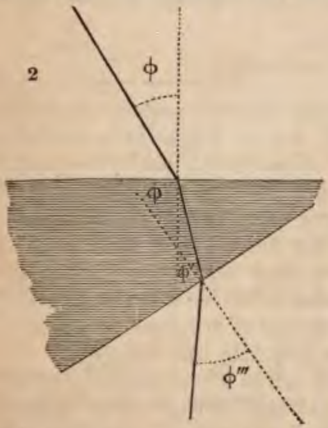
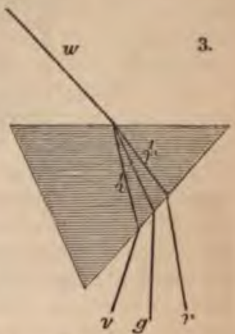


But it was soon found, especially by this last mode of observation, that besides *deviation* another phenomenon is produced: viz., *colour;* that is to say, that if a ray of ordinary white light enter a dense medium, it is separated into certain component parts, which give sensations of different colours: this is the case in a very small, indeed quite insensible degree, at one *refraction*, but in two at inclined surfaces, as in fig. 2, it becomes per*fectly* conspicuous. The progress of the effect is represented in fig. 3, where a very slight separation of the ray into component parts at the first surface, throws them at different angles on the second, and thus each is then still more deviated, but each by the same invariable law, and to a constant amount peculiar to itself, *w* is the white light incident, *r* the red, *v* the violet, and *g* the green rays, with other intermediate tints, as they finally emerge from the prism. In other words, each of the primary component rays of light has a refraction different from the others; peculiar to itself; constant for the same medium; and differing from one medium to another. Thus, prisms of different substances refract the several rays in different degrees both absolutely and relatively. The general fact was the great discovery of Newton; the particular variations in different substances have been traced by his successors.



3.

Newton observed, "that to the same ray ever belongs the same refrangibility," but he employed only prisms of flint-glass, and perhaps one or two other substances. His successors soon found, that among different media there existed a vast diversity in the extent to which the effects were displayed. It had been already found that similar prisms of different substances would produce a very different total amount of deviation: that is, they were said to have different *refractive powers;* thus, in fig 4, a prism is represented which produces a greater absolute deviation of the whole body of light than that in fig. 5. But it was not till some time afterwards discovered that the extreme rays of the spectrum are more widely separated, or there is a greater *relative* deviation in some media than in others, as in the prism of fig. 5, compared with fig. 4. This is called greater *dispersive power:* these two powers bear no fixed proportion to each other in different bodies, though it is true, among a considerable number of substances, that the more highly refractive are also the more highly dispersive; but this is by no means universally the case.

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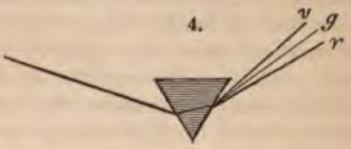

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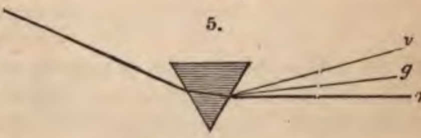

Besides these distinctions there was also another which was rendered manifest as observation extended: viz., this; that if two media differed in dispersive power, they did not by any means cause the different rays of the spectrum to deviate in degrees *proportional* to the whole amount of dispersion. Thus, suppose one medium, as in fig. 6, caused the green ray *g*, to take a position halfway between the

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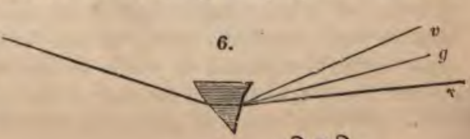

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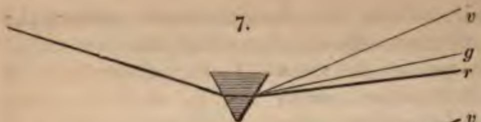

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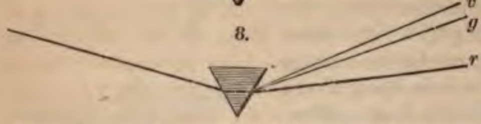


extremes r and v; another medium fig. 7, may give it a position much nearer to the red, and a third, fig. 8, much nearer to the violet end of the spectrum. There exist almost unlimited diversities in the *character* as well as the *amount* of dispersion in different substances; this was sometimes called "the irrationality of the coloured spaces."

The subject attracted the notice of several eminent philosophers, who devoted great attention and skill to determining by observation the amount and general character of the refraction and dispersion of a large number of transparent bodies. In this inquiry by far the most eminent for the extent and accuracy of his researches was Sir. D. Brewster; he devised the most ingenious methods of observation, and published extensive lists and tables of the indices of refraction for a long range of media, as well as of the differences in refraction between the extreme rays in each, and the ratio between this and the mean refraction; that is, in other words, their dispersive powers. No one can even inspect the tables in which the results of his labours are contained, without a conviction of the immense labour bestowed on the investigation, as well as of the curious insight which these very marked characters in different bodies, afford us of their specific peculiarities; of the unlimited variety which pervades nature in these properties; and of the extreme imperfection of our present knowledge of the causes on which it depends.

THEORIES OF LIGHT.

When philosophers began to turn their attention to the theories which might be invented to explain the phenomena of light, they were easily able to devise principles which sufficiently well account for the ordinary, simple refraction of a ray, on passing out of one medium into another, and even express accurately the law of refraction. The two great rival theories were those which are called the "corpuscular" or "emission" theory, and the "undulatory." The former was adopted by Newton, not as a real exposition of the principle of nature, but only as affording convenient mathematical methods of investigation. The latter was the idea of Huyghens, who illustrated it by the familiar comparison of the circles which spread themselves on the surface of still water when a stone is thrown into it. In this way he conceived extremely small motions, or waves, to be propagated through an excessively rarefied medium, or æther, which pervades all space and all bodies; these waves or pulsations, striking upon our organs, produce the sensation of sight. The former, or corpuscular theory, supposed extremely minute particles shot off from luminous bodies in straight lines in all directions.. Upon either of these theories it could be shown *why* a ray of light should deviate from its course on entering a new medium; but upon the undulatory hypothesis this was connected with a wider range of phenomena;

and even in the hands of its inventor, Huyghens, it had far outstripped the rival theory in its applications to important physical laws.

When, however, the unequal refrangibility of the primary rays of light was established, both theories seemed equally at fault; both seemed alike incapable of affording any explanation of the fact, even of the most general kind; indeed, in the then stage of the inquiry, no explanation but the *most general* could have been attempted; as no *accurate* knowledge of the facts, nothing like *precise data*, much less any mathematical *laws* had been obtained. Nay, even more than this was soon apparent; for the undulatory theory (as commonly conceived) not only did not explain, but seemed absolutely contradictory to the fact of unequal refrangibility. Upon that theory (in its ordinary form), the *equal* refrangibility of all rays was a *necessary* consequence. The rival theory was equally difficult to reconcile with the phenomena. But both theory and fact were as yet imperfecty developed.

The grand step (in reference to our present subject) in the improvement of the former, was made by M. Cauchy; of the latter, by M. Fraunhofer. Of the improvement and extension of the *theory*, we fear it would be an utterly hopeless task to attempt to convey any notice to our readers within the compass of an article like the present. We must satisfy ourselves by merely observing, that if the propagation of circular waves on the surface of still water be adhered to *as an illustration*, the velocity with which these circles succeed one another will depend on the density of the medium; and in the theory of light, the velocity with which the waves producing light succeed each other (though inconceivably great), is subject to certain changes, and is invariably diminished in more dense media. It is, indeed, owing to this diminished velocity, that refraction is shown to take place, and it is the measure of the refractive power. On the ordinary theory, whatever might be the lengths of waves, they would all have the same velocity in the same medium. M. Cauchy's grand improvement of the theory, consisted in so modifying it, that while it still continued to fulfil all the conditions it did before, it also assigned an explanation for a change in the velocity corresponding to a supposed difference in the length of a wave.

Now, on the same theory, the characteristic difference of the several primary rays is, that they are produced by waves of different, but determinate, *lengths*. The relation, therefore, established by M. Cauchy, assigned a connexion between the velocity, that is, the refrangibility, of a ray, and the length of its wave, that is, its colour.

The improvement in the investigation of the *phenomena* consisted in several particulars. In the first place; Dr. Wollaston, in 1802, had pointed out, that when the spectrum is formed, taking a very narrow line of light as the origin, the coloured spaces appear crossed by several parallel dark bands. M. Fraunhofer, in 1819, without knowing of Dr. Wollaston's discovery, observed the same thing, but with much superior apparatus; and thus was enabled greatly to extend the minute knowledge of the nature of the phenomenon. Instead of a few, he found, by a telescope, an almost infinite number of such lines or bands. What their nature *might be*, *there* was no means of conjecturing; but, from a variety of experiments, they appeared something inherent in the nature of the light. Seven principal, and well-marked, lines, were fixed upon as identifying determinate points in the spectrum, and were called *the standard rays.*

DISPERSION. COMPARISON OF THEORY AND OBSERVATION.

THE observations of Newton and his successors had hitherto referred only to the different refrangibility of what were termed, in a general way, the red, blue, &c., rays. These increased in refrangibility as we advanced towards the violet-end of the spectrum. The differences in the effect produced by prisms of different substances, were estimated generally by taking the refractive indices for the red, violet, and mean rays: the latter only were observed; the extremes inferred and calculated. The determinations were necessarily of the most vague and uncertain kind, since there was no precision in the definition of the rays. It depended only on the judgment of the eye to say (for example) how far the red should be considered to extend, and where the yellow should begin; and what point of the red, yellow, &c., should be taken as the point of measurement.

Fraunhofer, however, having obtained the means of more exact definition, by means of the fixed lines, proceeded to make use of them for affording a basis of exact measurement of refractive indices. He, accordingly, observed, with an extremely delicate apparatus, the deviations of these precise and well-defined parts of the spectrum, and thence deduced, by an easy calculation, the refractive index for each of the seven standard rays.

This he did for 10 substances, of which he formed prisms, viz.:—4 kinds of flint-glass, 3 of crown-glass, water, solution of potash, and oil of turpentine. These determinations are justly esteemed as amongst the most valuable optical data we possess; and it is, on all hands, evident that to have such precise numerical results, is the first essential preliminary, before we can attempt any philosophical investigation of laws or causes.

Further, there are several optical phenomena, by which not only the existence, but even the precise magnitude, of the waves, or *lengths of undulations*, are determined. These had been assigned, in a general way, for the red, blue, &c., rays by Newton. Fraunhofer determined them accurately for each of the seven standard rays. Now we thus possess two distinct sets of numbers belonging to the same standard rays,—their *lengths of waves*, and their *refractive indices*. And the first and obvious question which arose, was, Can any relation be traced between these two series of numbers? The first are independent of all particular media; the second are different for each different medium. An inquiry, then, into any relation which may be found between them, would be the first requisite before we attempted to venture on any theory.

Now such an attempt was made, in 1827, by M. Rudberg. He examined the sets of numbers given by Fraunhofer, and, upon trial, deduced a conjectural rule, or empirical formula, assigning an arithmetical *relation* between the length of a wave and the refractive index. He *found that the* calculated numbers agreed very closely with observation throughout the whole of that particular series. These results, however, were simply empirical and unconnected with theory. And it still remained to see whether any such rule could be deduced *from theory*, which should stand the test of comparison with observation in the cases already determined, and still more, in the vast number as yet unexamined, but which constitute so fine a field for the researches of future observers.

Now, from what we have already said of M. Cauchy's investigations, it will be apparent that they contain the germ, as it were, of such a rule or formula. The deduction of it was suggested by Mr. Airy (now Astronomer Royal), and developed by Professor Powell, in some papers in the *Journal of Science*, in which he had given an abstract of M. Cauchy's researches, and still more recently, in the same journal, a continuation, containing the investigations of Sir W. R. Hamilton, to facilitate the application.

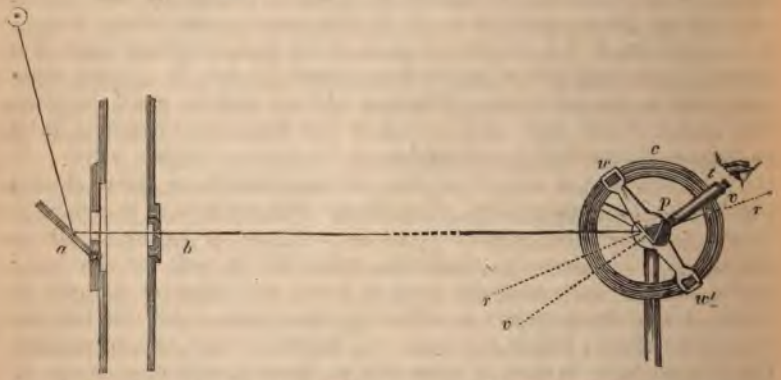
By pursuing the calculations from every one of the cases determined by Fraunhofer, Professor Powell succeeded in verifying completely the theory, as *far as those cases are concerned:* the results are given in a tabular form, in the *Phil. Trans.* for 1835, part I. Ten other cases had also been examined experimentally, and, in each, the seven refractive indices found, by M. Rudberg. These valuable data were also compared with theory, with as perfect success as the former, by the same author, and the results are printed in the *Phil. Trans.*, for 1836. *Thus for 20 media, including a considerable range of refractive and dispersive powers, a formula deduced from the undulatory theory as modified by M. Cauchy, is found to give a very close approximation between the indices calculated, and those determined by actual observation.*

Now, among these substances, the highest in refractive and dispersive power are by no means the highest in nature. On casting the eye over such lists as those contained in Sir D. Brewster's optics (*Cab. Cyclop.*), or other works, it will be seen that there are many media of dispersive powers, much higher than any of those above alluded to. Again, those who examine the mathematical formula will see, from its particular form, that though it may apply well enough to low dispersive substances, it by no means follows that it will hold good for those of higher power. It therefore becomes a subject of the deepest interest, to carry on the research for those substances. The theory may yet have to be modified, before it can be truly applicable to the real case of nature in all its generality. The formula which has succeeded so well for the lower media, MAY be only a simpler case of some more complex formula, to which it may be necessary to resort for the higher. All this remains to be investigated; and in this research Professor Powell is now engaged. The first thing to be done is obviously to obtain good measurements of the indices of the standard rays, for the several highly-dispersive media.

No such determinations are at present known to have been made. Should any such be ascertained, the publication of them will be a valuable contribution. The last-named author has investigated a few. At the Dublin meeting of the British Association, he mentioned some results of t kind, which he had then obtained (confessedly only rough approximatio for the very highly-dispersive substances, oil of Cassia, oil of aniseed, sulphuret of carbon. These were hardly worthy of comparison theory; but, as far as they went, there was a sufficient general accordance to render the further prosecution of the subject a matter of the highest interest. In the way of this research there exist considerable practical difficulties. For example, the *highest* known dispersive medium, *chromate of lead*, in the purest form of crystallization in which it can be procured, it is feared, is never sufficiently clear and homogeneous to afford such a prism as will show the fixed lines: the best determinations we can obtain will probably be but vague.

Again, oil of cassia has the property of totally absorbing the violet rays; so that that end of the spectrum is wanting in a prism of this medium This is the case also with some other liquids. So long as the medium is a transparent fluid, it can be easily examined, by being contained in a hollow glass prism. If it be even semi-fluid, or of any such consistency that it will easily take the mould of the prism, and retain a pellucid and homogeneous character, there is no difficulty in observing the deviations of the several standard rays by a graduated instrument. Solid substances, sufficiently clear, and cut with two inclined faces, may be used in the same manner, even if but of very small size. Many, however, of the most highly-dispersive substances, are of such a nature as to preclude the application of this method; and it is difficult to see how they can be subjected to examination at all. Those of them which can be dissolved in liquids, alter materially the dispersion of those liquids; but we are in possession of no principles on which to *infer* the amount of effect due intrinsically to the substance in solution. Solutions of the salts of lead afford good instances of this.

The general nature and arrangement of the apparatus for these determinations will be readily understood from the annexed sketch.



For seeing the *extreme* rays of the spectrum the sun's light is necessary: that of the clouds suffices for the others. Hence the usual apparatus for throwing the sun's rays into a room (by an inclined plane mirror) is employed, which is represented at *a*, the rays pass through a narrow slit *b*, and at a considerable distance fall on the hollow prism *p*, filled with the medium under examination, whence the spectrum *r*, *v*, is viewed by a *small telescope t*, with cross-wires in its focus, and moving on an arm *about the centre* of the graduated circle c, where the prism is fixed, with slight motions to adjust it to parallelism with the slit, and the position of minimum deviation; the indications which measure the angle of deviation for any given ray, from the direction in which it falls when no prism is interposed, are read off by two opposite verniers, *w*, *w′*.

PROFESSOR WHEATSTONE'S EXPERIMENTS.

FRAUNHOFER, in his original observations on the nature of the prismatic spectrum, and the dark lines which interrupt it, had remarked that the appearances were essentially different for light *from different sources*. The same thing had been still earlier noticed in the case of flame by Dr. Wollaston. The light from the main body of the flame of a candle, received through a narrow slit, and examined by a prism of flint-glass, gives only one bright line corresponding to the line D in the solar spectrum: but when the lower or blue part of the flame is used, the appearance is that of several coloured bands, with wide dark intervals. Fraunhofer found the light of the moon and Venus give lines exactly the same as those of the sun: the brightest fixed stars produced others peculiar to themselves. And electric light again had a number of bright lines of a very peculiar kind. To extend this series of results in certain highly-interesting cases was the object of a number of experiments lately carried on by Professor Wheatstone of King's College, London.

He examined, in all cases, the spectrum formed by a prism of flint-glass, by means of a small telescope; and varied successively the source from which the light was obtained. He repeated Fraunhofer's observation on the electric spark, and found that according to the different nature of the metals from which the spark was taken, the bright lines varied in number and position. When the spark was produced between two dissimilar metals, the lines belonging to both were simultaneously seen.

He further prosecuted this research by examining the light of the Voltaic spark taken between different metallic wires, connected with a powerful battery, and found a number of curious distinctive peculiarities. In general they were essentially different from those which mark the results derived from common electricity.

When the source of light was the luminous line obtained from the *electro-magnetic* spark, Mr. Wheatstone observed a variety of new and singular facts. These were principally elicited by employing different metals in the combination, and it was found that, according to the nature of the metal from which the spark immediately emanated, the spectrum formed was different in several remarkable particulars. In all cases, however, where electro-magnetism was employed, the spectrum presented this peculiarity: instead of a continuous succession of tints or coloured spaces, interrupted only by the faint dark bands which are seen in the ordinary spectrum, it was in all these instances completely interrupted by broad dark spaces, so that it was, in fact, reduced merely to a certain number of isolated narrow bands of light of different colours, at wide distances from each other. The nature of these bands, and their intervals, were the subject of accurate examination and comparison in Mr. Wheatstone's experiments.

When the metal employed was mercury, seven bright bands were seen. None of these corresponded to the red end of the spectrum, the first or lowest being orange and double, then green; next blueish-green, double; then purple and violet. The spark taken in the same manner from zinc, cadmium, tin, bismuth, and lead, in a melted state, gave similar results; but the number, position, and colours of the lines, varied in each case. The appearances were found so distinctly characterized that this method affords means of immediately detecting the presence of any particular metal. The spectra of zinc and cadmium are marked by the presence of a *red* line, which is wanting in all the others.

In all these cases the spark was produced (as we stated) from an electro-magnet: when the spark of a simple Voltaic pile was taken from the same metals, still in the *melted* state, the appearances were found precisely the same. And they were accurately compared by being viewed through a telescope furnished with micrometer-wires.

The Voltaic spark from mercury was taken successively in the ordinary vacuum of the air-pump, in the Torricellian vacuum, in carbonic acid gas, &c.; and the same results were obtained as when the experiment was performed in air, or in oxygen gas. Hence an important inference resulted, viz., that the spark, and the modifications in the nature of its light, were *not* due to *combustion* of the metal. By way of further elucidation of this point, Mr. W. examined by the prism the light which accompanies the ordinary combustion of the metals in oxygen, and by other means, and found the appearances totally dissimilar to the above. He concluded, therefore, that the sparks cannot result from the combustion of the metals employed in the Voltaic combination, but rather from a portion of the metallic conductor, which is carried off by the electric discharge, and ignited.

In the foregoing sketch we have not attempted to advert to those very extensive and profound researches into the mathematical part of optics, which have been carried on by Sir W. R. Hamilton, Astronomer Royal of Ireland, and by that highly-gifted individual, Mr. Maccullagh, of Trinity College, Dublin, whose recent elevation to the Chair of Mathematics in that University, affords so gratifying an instance of the recognition of distinguished merit, and promises so valuable an accession to the scientific efficiency of the institution. This gentleman, at a period of life when most mathematicians are but commencing their studies, has already enriched the theory of light with some most important investigations, for which we must refer our readers to the *Transactions* of the Royal Irish Academy: in whose volumes the papers of Sir W. R. Hamilton are also contained.

LATEST RESULTS OF RESEARCHES ON THE TIDES.

In our first number we gave an account of the progress of discovery on this interesting subject. We remarked how rapid its advance had been of late years, compared with the almost entire neglect it had experienced in past times. This advance has continued; and even during the short interval since our former account appeared, some important additions have been made.

Both Mr. Lubbock and Mr. Whewell have produced additional papers on this subject, containing, for the most part, the details of a vast mass of observations of the tides, especially at Liverpool, with the calculations necessary for comparing them with theory.

The general theory we attempted to explain, as far as it admits of popular exposition, in our first number. Its main feature is the determination of the position which the waters of the ocean would assume under the united influence of the attractions of the solid earth, of the sun, and of the moon; and in which, under the joint influence of these forces, they would be in *equilibrium*. Hence, it is designated "*the equilibrium-theory*." The vast variety of modifying circumstances which produce so great a diversity of effects in the course of the tide-waves, as conveyed into the different smaller seas and channels, are, of course, to be ascertained only by careful observation. But the general nature of the effect they produce, must be taken into account in following out the principles of theory into all their ulterior consequences. By investigations of this nature, compared with long-continued series of observations at different stations, we are enabled to decide whether the equilibrium-theory is a sufficient and satisfactory explanation of the actual phenomena of nature. The *general* agreement between theory and fact has been already noticed, as quite sufficient to warrant us in recognising these as the main causes to which the effect is due. But there still remained many lesser details to be followed out. Now these have been recently the subject of examination; and Mr. Whewell has succeeded in reducing to an exact comparison so large a mass of results, that little doubt can now remain as to the competency of the theory to explain even the more complex modifications of the phenomena. Recurring, then, to the principles fully explained in our former paper, we may fairly presume that we shall be intelligible to our readers, in the use of the terms employed in these investigations; and shall deem it needless to offer any further illustrative remarks, but at once lay before them the following brief abstract of the conclusions at which Mr. Whewell has arrived.

MR. WHEWELL'S RECENT RESEARCHES ON THE TIDES.

The results of the recent researches upon the tides has been to show that the circumstances of the phenomena may be most simply represented by means of the *equilibrium-theory*.

If the waters of the ocean had at every instant the form of equilibrium, which would result from the joint attraction of the sun and moon; and if the earth were to revolve on its axis, there would result, in large seas, a tide at every place twice a day. The high water would occur at a short interval before or after the moon's transit; and this interval, and the height of the tide, would each go through a cycle of changes in the course of a semi-lunation.

But actually, the tides at every place happen as if the form of the waters was *behind* this equilibrium-form, both in *longitude* and in *time*.

The actual form follows the equilibrium-form at a certain distance in *longitude;* and hence the tide does not happen at the time of the moon's transit but a few hours *later:* this distance is the *retroposition in longitude*.

The actual form agrees with the equilibrium-form, not at *the moment*, but at a certain *antecedent* moment; and hence the highest tide is not at the new or full moon, but a day or two *later*. This interval of the two moments is the *retroposition in time*.

The changes in the intervals of the moon's transit and high water, during the cycle of a semi-lunation, and the changes in the heights of high water during the same cycle, agree to a remarkable degree of precision with the equilibrium-theory; that is with the equilibrium-form thus *retroposited*.

The changes thus spoken of, are those which belong to a mean parallax and declination of the sun and *moon*

But by recent examinations of tide-registers, it appears that the effects of *changes in the parallax* and *declination* of the moon may also be explained with great accuracy by the equilibrium-theory. The following propositions have thus been established.

The interval of the tide and moon's transit is affected by changes in the parallax and declination of the moon, according to the same law, and by nearly the same quantity, as it would be in the equilibrium-theory.

The height of the tide is affected by changes in the parallax and declination of the moon, according to the same law, and nearly by the same quantity as it would be in the equilibrium-theory.

With this additional change:—

The *Retroposition in longitude* undergoes a small alteration, which is proportional to the effect of the change of the parallax, and also a small alteration, which is proportional to the effect of the declination.

The retroposition of the tide in time also undergoes a small alteration of the same kind.

According to the equilibrium-theory, the two tides on the same day should differ by a *diurnal inequality*, both as to their height and time.

This diurnal inequality had never before been introduced into tide-tables. But Mr. Bywater, in his *Liverpool Tide-table* for 1836, has obtained from a collection of observations, the amount and law of this difference, and has made use of it in calculating his tables, so as to render them more exact than before.

DESCRIPTION OF

THE GREAT CAVE OF GUACHARA,

IN THE PROVINCE OF CUMANA.

Translated from the Spanish of Colonel Coadacia, of the Venezuelan Army, now employed by the Government in making a Map of the different Provinces of the Republic.

[*From a Correspondent in South America.*]

The small town of Caripé is situated in lat. 10° 11′ 14″ N., and long. 3° 3′ 45″ to the east of the meridian of Caracas, in a valley formed by the waters of the River Caripé, which flow from the summits of the Purgatorio, Guacharo, Periquito, the table-land of the Guardia de San Agustin, and the peaks of Caripé. The vale is at an elevation of 961 varas (Spanish yards) above the level of the sea, enjoying a moderate temperature both in summer and winter, during which seasons the thermometer is commonly from 18° to 20° (centesimal). The wild camomile, borraja, mint, and tobacco, grow; also fine cabbages, garlic, fennel, and onions, together with exquisite coffee; in fact, the only thing wanting to make this little valley extremely rich in every way, is a Creole population,—at present, it is solely inhabited by Indians of the Chaimar nation, who have not yet been able to abandon their native character for one more active, that might render them more useful to the state, by destroying that inflexible apathetic indifference, which will not allow their participating in the more civilized benefits, as well as prosperity, enjoyed by their fellow-countrymen.

The village consists of a few miserable straw huts, of a small church, together with one or two long lines of hovel adjoining to each other; the whole covered with tiles, in consequence of having once been the residence of Capuchin missionaries. In this primitive monastery was a chapel, now in ruins, the front walls of which now only remain. The view from this spot is not unpicturesque. The small river of Caripé, bordered by innumerable trees, flows in a narrow bed and serpentine direction (from W. to N. E.) through the valley, ultimately pouring its waters into the river St. John; when thus united, they become tributary to the Golfo Triste. To the north frown the highlands of San Bonifacio, joining those of Guacarapo, surmounted by an enormous mountain. To the west rise those which encircle the Table-land de la Guardia de San Agustin, and also those which unite with the chain of the Guacharo, over whose summits towers the naked peak of "El Purgatorio," elevated 1852 varas, based in the bosom of a thick forest, which extends itself nearly to Santa Maria; at some distance on the opposite side, rise several isolated conical-looking rocks, almost inaccessible, forming the barrier that divides this valley from the waters which flow to the south, and unite with those of the river Guarapiche.

On the 1st of February (1835), I arrived at Cumana. The second was passed in making several observations, whilst, in the mea time, twelve Indians had proceeded towards the skirts of the high lan of "*El Purgatorio*" *to* cut palms (called Palmiche), in order to form the flambeaux intended to light us whilst visiting the cavern. The following was the plan adopted.

The palm-tree in question commonly grows to the height of twenty feet, having leaves like that of the corozo, and its bark, like that of the wild cane, is without thorns, but possesses an infinity of knots, which however do not interrupt the vertical and straight direction of the fibres within the body of the tree. These palms are generally from three to four inches in diameter; and on the bark being taken off, their trunks were split from top to bottom (following the direction of the fibres), into pieces of about two yards in length, and half an inch in thickness. This operation done, a fire was kindled, formed of a quantity of fagots; upon it several pieces of wood were laid, supported by forked sticks, and the whole of them became perfectly dry in less than twenty-four hours. Each of the palm-slips were then placed between some of the before-named pieces of wood, in order to form the required flambeaux, which, when completed, might be nearly six inches in thickness, the whole was bound together with a few turns of the bejuco (a rope-like runner). One of these torches would burn for nearly an hour in the cave, spreading a great light; and that without giving any bad smell, notwithstanding it emitted as much smoke as any other wood generally does.

On the 3rd, our party set forward to the Cave. It is two Colombian leagues to El Poniente, following the edge of the high-lands of the Guacharo across a plain, on our way we crossed three times the small river that flows from higher grounds, which is the only stream that passes near the Cave, where it narrows exceedingly between the base of the Guacharo and the Periquito: the road here is bad and rocky, and also traverses a thick wood. Suddenly the entrance of the great cave opened upon us, where we found the Indians busied in preparing the flambeaux.

Our instruments consisted of a thermometer, a compass, a sextant and stand, chronometers, and a barometer. We took a cord of 20 yards in length, and began our measurement from the entrance of the cave I am about to describe. It was at half-past eight in the morning we entered it, and we re-issued from it at a quarter-past eleven, after getting well soaked in the stream that flows through it. At mid-day I took a solar observation at the mouth of the cavern, to ascertain its longitude, latitude, and elevation above the level of the sea.

The cavern of Guachara is situated in the bosom of a mountain from which it takes its name, at an elevation of 1739 yards above the level of the sea. The Guachara is composed of an alternate secondary formation of calcareous rock, and occasionally argillaceous chalk, and other calcareous alpine masses, or like those of Mount Jura: the mountain's exterior appearance is round all the way to its base facing the east and south-west. Its northern flank rests on the table-land "de la Guardia de San Agustin," and to the north-west it joins the dark range of the "Purgatorio;" to the south it presents a base steep and rocky. On looking on the huge elevation in question from this spot, for about two-thirds of its height it shows a ridge entirely covered with long grass, accompanied by numerous minor elevations, as well as gigantic masses of rock rising vertically in different groups: the crown of the mountain presents nothing but naked rocky precipices, broken into huge ravines, filled with shrubs and stunted trees.

If enormously-spacious caves mark the general character of calcareous mountains, it is therefore nothing so very extraordinary that one so extensive should be found in the heart of the "Guachara." This cavern is in latitude 10° 11″ N., and in longitude 3° 32″ E. of the meridian of Caracas. Its elevation above the level of the sea, 1170 yards,—consequently higher than Caripé by 229 yards. Its mouth fronts the S. S. W. forming an opening of 31 yards wide and 28 high; and its intricate windings follow a direction to the N. N. E. The appearance of the cave presents to the eye a complete arch, opening on to a wide ravine running directly from it westward; at a few paces distant rise piles of petrifactions, forming countless grottoes, stretching away in the line of a vast amphitheatre, till lost on the great plain amidst its marshes and aquatic plants that crowd in rank luxuriance over its surface.

From the arched roof of the cavern hang large masses of ancient stalactites, both great and small, some from 12 to 14 feet in length, and from 3 to 4 in thickness; many are sharp-pointed, some oval-shaped, others spherical; in fact, the whole so beautifully put together, they might be taken for the most perfect work of art, instead of the effects of petrification. We advanced 115 yards, where the bottom of the cave then became somewhat muddy: it was here not more than 25 in width and 22 in height, still holding the same general direction, as well as form. We had hitherto only carried one torch; but on arriving at 175 yards, the obscurity became so great as to compel our lighting four others. Whilst thus occupied, our ears were suddenly assailed by the noise of the Guachara birds from the deep interior of this gloomy place. The first saloon, if so I may call it, is divided in the middle by a large mass of petrifaction; and from the point to which we had arrived, branched off a second way, resembling, to every appearance, the one we were now exploring: here vegetation ceased; the whole surface of the cave's bottom was covered with rotten fruit, called *mataca*, eaten by the Guacharas. This fruit is of a red colour, the produce of a tall straight tree growing on the neighbouring mountains; and it is only during the night that these birds leave their dark retreat in search of it, in order to feed their young, safely nested within the rocky bosom of the earth. It would seem that, when the bird has digested the pulpy part of the fruit, it returns the seeds of it, after their having remained a certain time in the stomach, from which, the Indians say, they acquire a particular virtue, as a remedy for spasms, cholic, and intermittent fevers. At certain periods the natives gather these seeds, string them, and hang them up in their cooking-places, because the smoke dries and preserves their medicinal properties;—two or three of the seeds mashed, or well-bruised, put into tepid water, form the dose to be given.

We continued our observation and measuring; advancing, whilst the affrighted and restless Guacharas flew screaming in myriads abo our heads in the obscure arch of the cave. At the distance of 240 ya *our way began to rise* at an angle of 25, over petrified calcareous ro whose ledges were perfectly honey-combed. At 325 yards we descended a little over an easy slope of soft earth, formed from the decayed deposits of the fruit on which the birds had fed, as also from the immense quantity of the dung created by them; so abundant were they at this spot. The colours of the "Guachara" are equal in beauty to those of the "Guacharaca," in the plumage of them are seen several white circles—the head resembles that of the "Guarda-camino," with a sullen melancholy look in the eye—it makes a noise like a roaming cat, screaming even more sharply. We here found a sort of *primitive ladder*—a fixture, left by the Indians—serving as the means of taking the young birds from their nests for the sake of their fat. It consisted of a stout and long pole, of the "Mataca" tree, placed almost vertically, and supported at the foot upon several pieces of wood, held fast by four stakes—the top rested against an incrustation of the cave—the pole had various cross sticks at certain distances, serving as rests for the feet of the mounter; at the upper extremity was firmly attached a "*bejuco*," and additionally so, to the more solid of the petrifactions, forming a sort of cord, stretched beneath, and not very distant from the arch. The Indian, after ascending to this part of the machinery, places his feet (like a rope-dancer) on the bejuco, guiding himself by one of his hands, grasping the projecting incrustations; and with the other, on reaching a nest, draws forth the young birds. The month of June is the period for collecting the Guachara fat, which is extracted by frying the little creature. What is produced by this operation, is like the most exquisite and delicate-tasted butter.

The cavern maintains an almost equal direction, as also height and breadth; nor does it vary much even at the numerous points where the fantastic incrustations and stalactites rise so multitudinously. Veins of gypsum are found with those of the calcareous formation of Jura, or with that of the Alps. Sometimes these two formations are together, sometimes separate; and often repose between the Alpine calcareous formation and the argillaceous chalk—The branching subterraneous hollow before mentioned, as branching off to the right, we again met with crossing the cave; but within a short distance, both it and the stream of our own became lost amongst masses of petrifactions; and from a distant roaring noise, it seemed as though the waters of the cavern had accumulated, and were rolling over some awful precipice, forming an invisible cataract. The temperature of the air, in this place, was between 18.5° and 19 centesimal, whilst the external atmosphere of the cave was at 17.5°, and within the mouth we left it at 18°. The celebrated Baron Humboldt, in the month of September, 1800, found the air of the cave 18.4° and 18.9°. The external air at 16.2°—within its entrance at 17.6°; and on putting the thermometer in water, it stood at 16.8°. He adds that these experiments offer great interest, when it is considered that heat always maintains its equilibrium, between the water, the air, and the earth. I put the thermometer into the stream, where it disappeared, and it gave a difference of 2° colder than the air.

On arriving at 570 yards, the ground took an inclination of 60°. Beyond this distance Humboldt had not proceeded—nothing could induce the Indians to move a step further—neither entreaties nor promises.

The frighful aspect of the cave had so filled them with ideas of terror, as well as the loud screechings of the millions of birds; but above all, they were horror-stricken by an impression that this dark and dreary spot had of old become the refuge of their persecuted forefathers; and fostering such fears and thoughts, they would go no further. But the Indians of the present day are not quite so nervous or sentimental, therefore they followed my steps without hesitation. We scrambled up some of the honey-combed rocky masses, closing the ascent at 632 yards;—here the cave is not more than ten wide and about 12 in height, the general direction the same as before. We soon re-met with the before-named ravine, or hollow, and followed it, until stopped from advancing further, by masses of pyramidal and vertical petrifactions. Having now reached to 647 yards, our obstructed way suddenly took a turn to the right, narrowing amongst a confusion of columns,—small cavities and crowds of stalactites and stalagmites rising in chaotic masses on every side; swarming with Guacharas, whose diabolical noise is inconceivable, as well as indescribable. We pursued our way to the right through the ravine, which was covered with the pulverized dung of these birds; it gradually and almost imperceptibly took an upward inclination to a point, where the cave became only 22 yards wide, but preserved the usual height, presenting the most grotesque and singular forms of petrifactions in all directions. At 885 yards the ravine became narrow indeed, so continuing to 950, when it suddenly came to an ascent on an angle of 70°, the way difficult and interrupted by great stalactitic masses.

The Bishop of Guayana, according to Humboldt, only got as far as this point, and there seems no doubt of his fully believing that the cave terminated here; and the Baron was of opinion it did not exceed 1088 yards in extreme depth. Beyond this spot the Indians of that period never had been, it being the last place where the Guacharas were taken. We mounted several heaps of petrifactions, nearly touching the roof of the cave, but could not discover any further outlet whatever. I looked at the barometer and found we were now 225 yards above the level at the entrance of the cavern. Somewhat to the east, at the foot of these steep masses of petrifaction, I met with an oval-formed well, whose diameter was from two to five yards, and in depth ten, having its sides perfectly vertical; by the aid of our torches we were enabled to perceive a narrow cave, taking a northern direction. We at first thought of taking the Indian wooden ladder, in order to lower ourselves into it (for it sunk like an abyss), should we not find any other arched chasm that might conduct us to our former ravine, which we had lost amidst the multitude of yawning cavities that presented themselves on all sides. We returned, therefore, carefully examining every opening for full 25 yards. We found one, not more than a yard in width and height, inclining to the eastward at an angle of 45°; its floor was covered with pulverized dung, and marked with the footsteps of animals, which at the moment we could not recognise as to what kind, but shortly afterwards they proved to be those of lapas. I asked the Indians whether any of them ha proceeded lower down this cave, but they said no, therefore co *serve as guides. I then took* myself one of the torches, and crept within this narrow passage, and continued creeping for some six yards, when it widened to twelve, and I was then enabled to get on my feet. A few of the party soon followed me, and as we advanced, the cave gradually augmented in size, as well as in declivity. At 25 yards we again re-met our sought-for ravine. The Indians who bore our store of flambeaux and ropes had not come forward with us; therefore one of us (the Alcalde) returned, and prevailed on four of them to follow down, leaving the rest with only two lights, to find their way back.

We pursued our way through this newly-discovered cave; finding we had good store of torches, both for going and returning, we advanced without hesitation—the way was very narrow, with a stream of water running through it, about four feet deep, over a soft and miry bottom; the sides steep and slippery, which rendered it difficult for us to keep safely on their edges. Everything here bore a totally different aspect to what we had seen hitherto—no stalagmites, no stalactites, nor incrustations of any sort—nothing either of a calcareous or gypsum nature existed; but one mass of argillaceous chalk, whose solution quite discoloured the water, formed this subterranean branch—not a trace of a bird could be discovered—the most deadly silence reigned; in fact the water stole away without a murmur; here the cavern was no more than 4 yards high and 12 wide. We continued along the edge of the stream with some degree of difficulty, for about 50 yards, having no other path to take; and here it was yet worse, becoming only a passage of 2 yards in width, and from 2 to 3 in height, having the water up to our waists; the bottom gravelly. Thus marching, we proceeded for 25 yards, when our passage began to widen to 3 yards, and elevate to 5. We held our way on the canal or stream for 125 more: here we were stopped by the water forming a sort of well, two yards in depth, four in length, and two in width—a small and very narrow opening showed us where the stream left this tiny lake. Any further examination would have been useless, in the attempt to pursue our way further up this uncomfortable branch of the great cave; up to this point it ran 225 yards. Here the thermometer stood at 19° 4″, and when immersed in the water 18° 5″. We now returned, and at 25 yards met with a nearly vertical fissure, of about 2 yards in width, from which trickled water; its direction was N. N. W. On entering it, for about 5 yards, it widened, rising on an angle of 45°. We passed over a calcareous and gypsum kind of path, not wanting in incrustations, and ascended for nearly 12 yards, when instantly the chasm narrowed in every way so considerably as to render it impossible to stand upright, and therefore we were compelled to creep on our hands and knees. But what was our admiration and surprise, when after three yards more, we suddenly rose in a magnificent saloon, containing three enormously-wide arches, one towards the south, another towards the west, and a third towards the north-east; here the hidden wonders of the earth were presented to us in every kind of the most beautiful petrifactions. The whole of the arched roof seemed to be of crystal, cut into the most exquisitely-fantastic and wonderful work the human taste can imagine; there were pendent stalactites as singular in their forms, as they were brilliant in their composition. Columns, pyramids, obelisks, white and silvery-gray, with a diversity of glittering veins, resembling an assemblage of works in alabaster, bronze, the purest marbles of various colours, sprinkled with diamonds, whose lustre shone resplendent whenever the light of our torches fell upon them. The floor of this enchanted-looking spot was covered with the most curious petrifactions, many really beautiful, whose points and regularity of sides could scarcely be surpassed by the most exquisite-formed brilliant. All the petrifactions were recent, and all presented an assemblage worthy admiration. In the middle of this splendid saloon, arose on semi-circular steps, a sort of tomb, resplendent as silver, and spotless as alabaster, about 3 yards in height, like a circular temple, formed in excellent proportions, terminating with a dome so perfectly shaped, that the most able architect could not have traced it with more exactness. On it rested a globe, surmounted by a small broken pyramid; to the left of this self-built edifice, rose two colums, not unlike those of the Ionic order, and so inimitably matched, that design, and not accident, seemed to have been the fact; they supported an arch, which together presented a bautifully-simple portal. The capitals and pedestals were of a brilliant silvery gray, whilst their shafts were white as snow; on the opposite side stood numberless pillars, bright in the same colours, without order, presenting to the eye a vast saloon, filled with the most splendid crystallized stalactites.

Every one of us remained gazing in silent ecstasy and surprise at these extraordinary works of nature. The Indians were scattered here and there, apparently immoveable, lost in admiration like ourselves, holding in their hands the blazing flambeaux, whose light beamed forth in various directions, displaying objects truly worthy to be described by a master-pen, and which no human pencil could possibly imitate,—the reflected brilliancy darting from the forest of pillars wherever the rays of the torches fell, the deep shadows enshrouding others, and the gradually dimming forms of hundreds more, losing themselves in the blackest obscurity; finally, the smoke rising perpendicularly, in volumes, towards the vaulted roof of this noiseless cavern,—all, all seemed as though we were spell-bound in some enchanted mansion.

After examining this wonderfully-singular scene, we took a north-easterly direction, over some semicircular steps, which led us into the midst of crowds of stalactite and stalagmite columns, where we beheld all the different stages of petrifaction and incrustation, from its first formation by the falling of each filtered drop, impregnated with muriate of gypsum, to its accumulated and solidly-fantastic figure. The height of this beautiful cavern is 18 yards by 14. Here we found no birds whatever; all was still as the tomb, and solemnly inviting to meditation. We continued advancing, still in ecstasy at its varied beauties and wonders, for 125 yards, where its vastness terminated; but still it as curiously continued rapidly diminishing, to an extreme narrowness, crowded up with every description of columnar petrifaction, besides an infinity of cavities and dark holes on each side. Notwithstanding the confined way we had got into, we persevered, going forward on our hands and knees; ten yards, however, of this, put a stop to any furth advance. We were compelled to return, and followed the cave to *west.* *At 30 yards* we met with every kind of stalactite impedim therefore returned again, and tried our better fortune to the south. Here we found vast masses of fallen incrustations. One enormous single column of a great length had given way, and was leaning on groups of truncated pyramids, at an angle of 45°; it seemed ready to fall upon us: the base on which it poised was an enormous petrified mass, of six yards in height. This branch of the cave rose considerably, and was based on broken calcareous rocks, which after an advance of 40 yards, became nothing more than a simple irregular cavern. From the position and direction of this subterraneous branch, we were of opinion that the oval well we had left in the grand hollow of the cave, at 950 yards, in whose depth of water was observed a small grotto towards the north, must communicate with the present; difficult certainly to be traced, in consequence of being hidden amongst the rocky projections described above.

We must therefore consider this extensive cave to be divided into three branches; the principal branch of which, 975 yards, is composed of ancient petrifactions, and inhabited by those nocturnal birds from which it takes its name. On turning back again for 25 yards, the second branch is met with, composed of argillaceous chalk, become hard from the constant action of the stream flowing through it, without birds, or any other living animal whatever; its length is 225 yards. On returning, as in the other 25 yards, the third branch is met with, inhabited by lapas*, being 135 yards in length, and certainly is the most beautiful portion of this extraordinary subterraneous wonder. Hence, these three branches united, form a total of 1285 yards; and the cave of Guachara doubtless ranks as the first of nature's singularities of that class—at least, known in Venezuela, and perhaps in both worlds.

P.

ON THE CORRECT MODE OF MEASURING ALTITUDES.

In our last paper we showed that it was of considerable practical importance to be enabled to ascertain, without any gross error, the altitude of mountains, in order to the determination if their cultivation would be profitable, or if their summits soared to a height beyond the limits of luxuriant vegetation. Besides this, there is nothing that would tend more to correct our ideas of physical geography, than to have the principal mountains in all countries measured with a tolerable degree of precision; and that the measurement is easy we will now attempt to show.

The different methods that have been adopted are the four following:—1st. By instruments capable of measuring vertical angles, or *geometrically*. 2nd. By means of the barometer. 3rd. By the temperature at which water boils at the base and summit of a mountain. 4th. By ascertaining the temperature of springs upon the spot, whose height is to be ascertained.

1st. The first method is both tedious and liable to several inaccuracies. A base-line and two vertical angles must be measured, and it *will be found* that, even with the best instruments, under the most favourable circumstances, and with the most practised observer, the determination of minute vertical angles is, from the influence of horizontal refraction, liable to much uncertainty. Besides, when the vertical angles are ascertained, a knowledge of logarithms and plane trigonometry is requisite, to ascertain the altitude; and although the calculation, to the initiated, is extremely easy, to those to whom our paper is addressed it would perhaps seem to be both intricate and involved.

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* An animal of the hare kind.

2nd. Torricelli, the celebrated pupil of the "starry Galileo," had no sooner made the important discovery that the column of mercury is sustained in the tube, now called a barometer, by the pressure of the atmosphere, than the famous Pascal subjected Torricelli's explanation to the severe test of measuring the height of the column at different altitudes, sagaciously arguing, that the mercury ought to descend, the higher the instrument was carried.

This crucial experiment was made on the Puy de Dôme, in Auvergne, and upon a high tower near Paris; and in both cases the column decreased according to the elevation of the instrument. Were the atmosphere of the same density throughout, as it is at the surface of the earth, the height, at the temperature of 55°, would be 27,600 feet; and at 32°, only 26,000 feet; but as we ascend, the air expands, from the diminution of pressure, and taking the altitudes in arithmetical, the densities are in geometrical, progression. In other words, in ascending $3\frac{1}{2}$ miles, we have passed through $\frac{1}{2}$ of the atmosphere; 7, through $\frac{3}{4}$; at 14 miles, the density is only $\frac{1}{18}$; or, were we to be carried up 14 miles in a balloon, there would only be $\frac{1}{18}$ of the atmosphere above us. From such a fact, it is evident that altitudes may be calculated by logarithms, because the terms of an arithmetical series are proportional to the logarithms of the terms of a geometrical; therefore, different altitudes above the earth's surface, which we have shown to form an arithmetical series, are as the logarithms of the densities, or of the weight of the air at these altitudes. Different formulæ have been given by different philosophers, for measuring the altitudes of mountains by the barometer, and among the rest by the illustrious Laplace, who, by certain refinements that he has introduced, has attempted to embrace the minutest anomalies of atmospheric pressure; but whether or not it may be considered a waste of the powers of calculation, to attempt to ascertain so minutely the volume of a fluid liable to such incessant fluctuation as that of the atmosphere; at least, the formula is too involved for us to attempt in this place to develop it.

We have shown that, at the temperature of 32°, the equiponderant column of air, of the mean density, as at the surface of the earth, would be 26,000 feet; or in other words, that such a column would balance 30 in. of mercury, and 32 feet of water; therefore, we have the following formula, which we take from Sir John Leslie; "as the sum of the mercurial columns at the bottom and top of a mountain is to their difference, so is twice the equiponderant column, or constant number, 52,000, to the approximate height of the mountain, in feet." We give an example.

If the barometer stand at 30·091 inches at the bottom of a m
tain, and 26·409 at the top, what is the height of the mountain?

$30{\cdot}091+26{\cdot}409 : 30{\cdot}091 - 26{\cdot}409 :: 52{,}000 :$ approximate heig
$56{\cdot}5 : 3{\cdot}682 :: 52{,}000 : 3442$ feet the approximate height

mountain. It must be recollected that this is only an approximation; but it would be almost correct, if the temperatures of the mercury and the air, both at the bottom and top of the mountain were 32°. Almost all bodies expand by heat; and therefore circumstances can be supposed, when, although the mercury would fall by being carried up a mountain, still, by an increase of temperature in the higher regions, this tendency to fall might be counteracted. And hence the temperatures, both of the air and mercury, become important elements in the calculation.

Mercury expands about a 9000th part of its bulk for each degree of Fahrenheit's scale above 32°, and hence a small addition must be made to the upper column when the temperature at the higher station is lowest. In a much higher ratio, however, does air expand; and hence the correction on that account is of more consequence. Its rate of expansion is one 450th part according to Leslie. To elucidate this by an example:—

In August, 1775, General Roy observed the barometer on Caernarvon Quay to be 30·091 inches, the attached thermometer being 60·26°, and the detached 60·08°; while on the peak of Snowdon, the barometer stood at 26·409 inches, the attached thermometer being marked 50°, and the detached 47·84, how high is the peak of Snowdon above Caernarvon Quay?

In the lower station, the attached thermometer is 28·26° above the freezing-point, and in the higher 18°, whose difference is 10·26°; hence multiplying 26·409 by 10·26°, and dividing by 9000, we obtain ·030 the first correction, to be added to 26·409; therefore

30·091 + 26·409 + ·030 : 30·091 – 26·439 : : 52,000 : approximate height; that is

56·530 : 3·652 : : 52,000 : 3360 feet approximate height.

In the lower station the detached thermometer is 28·08° above the freezing-point, and in the higher 15·84°, the half of whose sum is 21·96°; and if we multiply 3360 by this, and divide by 450, we obtain 164 to be added, and hence the true height of Snowdon, above Caernarvon Quay, is 3524 feet. The correction for centrifugal force, says Leslie, is 7 feet more.

In practice, however, it will be found sufficiently accurate to allow 940 feet for every inch that the barometer falls.

3rd. The temperature at which water boils, depends upon the pressure to which it is subjected. Under the receiver of an air-pump, tepid water enters into ebullition when the exhaustion is great. At the top of Mont Blanc, the highest mountain of Europe, which rises 15,630 feet above the level of the sea, water boils at 184°, or 28° lower than its boiling-point upon the shores of the ocean; and Laplace calculates that if $\frac{3}{4}$ of the water of the sea was converted into vapour, its pressure would be so great, that red-hot water might exist, and cover the primitive mountains of the globe (Cuvier's *Theory of the Earth*, Jameson's ed., p. 340). We have mentioned that under general circumstances the barometer falls one inch for every 940 feet of ascent. Now the boiling-point of water changes by a fraction, represented by 1·76°, for every inch that the barometer falls; and hence we obtain an easy rule for calculating heights, by observing the temperature at which water boils at the bottom and summit of the acclivity, whose height we wish to ascertain. For

940 ÷ 1·76 = 535 feet; and hence the varying of a degree in the boiling-point corresponds to an altitude of 535 feet, which is the number given by the Murrays in their *Popular Chemistry*.

We have never tried this formula practically, but Captain Styles, in the last January Number of *Silliman's North American Review*, states that he found this method sufficiently correct. In a few weeks, however, we shall have it verified, and the results shall be inserted in this Journal.

4th. The cold of winter, and the warmth of summer, only penetrate a few feet into the soil, without affecting the general temperature of the springs, which issue from the bowels of the earth. In every latitude there is a height where the temperature would only be 32°, or where snow would for ever lie. At the equator, the curve of congelation is 15,207 feet above the level of the sea; and at the pole, eternal winter reigns. From such facts, an empirical law has been deduced for estimating the altitude of a mountain, by observing the temperature of water, as it issues from its sides. "The allowance in this climate is one degree of Fahrenheit's scale for every 90 yards of ascent, and for every 100 yards in tropical regions." We have, therefore, only to ascertain the mean temperature of the latitude at the level of the sea, and the temperature of the spot, whose height we wish to ascertain, subtract the one from the other, and multiply by 270, and we at once ascertain its height.

We extract from Leslie's *Elements of Geometry* the following table of mean temperatures, which extend to the whole of Great Britain:—

Lat.		Fahr.	Lat.		Fahr.
50°	mean temperature at the level of the sea.	53·6°	55°	mean temperature at the level of the sea	49·2°
51°	,, ,, ,, ,,	52·7°	56°	,, ,, ,, ,,	48·3°
52°	,, ,, ,, ,,	51·8°	57°	,, ,, ,, ,,	47·5°
53°	,, ,, ,, ,,	50·9°	58°	,, ,, ,, ,,	46·6°
54°	,, ,, ,, ,,	50·0°	59°	,, ,, ,, ,,	45·8°

"The temperature of the Crawley and Black Springs, on the ridge of the Pentland Hills, near Edinburgh, were observed, says Leslie, by Mr. Jardine, where they first issue from the ground, to be 46·2° and 45°, which, compared with the standard temperature at the same parallel of latitude, would give 567 and 891 feet of elevation above the sea. The real heights found by levelling were respectively 564 and 882, a coincidence most surprising and satisfactory." To enter more minutely into this process:—

I found the temperature of the water as it issues from Beltow-grain vein, at Wanlockhead, to be 44°, and from this I deduced the elevation as follows:—

The latitude of Wanlockhead is 55° 20′, the mean temperature of which, according to the above table, is 48·87°; and hence the difference between the temperature at the level of the sea and at Wanlockhead is 4·87°. Again, 4·87° × 270 = 1334 feet, whereas by other means it has been ascertained that the altitude of Wanlockhead is 1330 feet.

After a full consideration of the subject, we con [illegible] last method of ascertaining the height of mountains to be by [illegible] *and from what we* have shown, it seems to be as accura [illegible]

A POPULAR COURSE OF ASTRONOMY.

No. III.

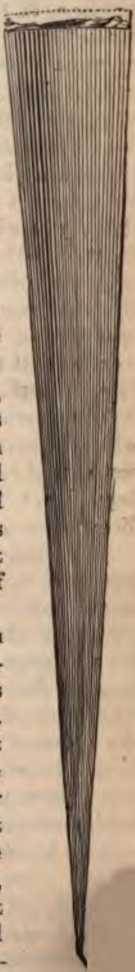
There is no manifestation of wisdom in creation more remarkable, perhaps, than this,—that a being so infinitely minute in the comparison as man is, should be rendered able to ascertain the form and dimensions of the huge mass on which he dwells, and of other worlds than this, of equal or greater magnitude, situated at distances in the space around him so great, that, large as they are, they are scarcely visible to him by reason of their remoteness. Let there be conceived an insect, less than the least ever seen with the naked eye, one of the animalcules to be traced by the aid of powerful microscopes in water, and let such a being be placed on a globe, a foot in diameter,—conceive this little being, not moving above the one-tenth of an inch over its surface, by the aid of an instrument, a thousand times less than himself, to make certain observations on the objects which surround this globe; and let this mite be endued with an intellect which enables him, from these observations, linking argument and argument, and piling conclusion upon conclusion, to say, positively, from that tenth of an inch of the globe, and that little instrument, what are the dimensions of the whole globe,—what is its circumference, and its surface, and its diameter, and its weight; and knowing these, to conclude from thence the dimensions and distances of all the other objects bearing any proportion to the magnitude of this globe, within ten miles round it; nay, to carry his speculations to certain conditions of the existence of objects whose distances from him must be measured by hundreds of miles. Let all this be conceived, and then let this globe be converted, in the imagination, into the mass of the earth, and the space round it into the heavens, and some conception will thus be obtained of the position which the astronomer holds in the universe.

In the last paper on Astronomy, it was shown that as an observer moves about on the earth's surface, his horizon *rolls*, as it were, under his feet; and moreover, (and this is a very remarkable fact,) that as he thus moves from one place to another, his horizon rolls through precisely the same angle which, if we imagine a line drawn continually from the earth's centre to his feet, that line would revolve through between the two places.

Thus, referring to the last diagram in the last paper, as our observer moves from A to B, a line imagined to be drawn continually from the earth's centre C, to his feet, will revolve through the angle ACB. Now the angle ACB is precisely equal to the angle through which his horizon has, during the same time, been made to revolve. If, then, we know or can find out one of these angles, we know or can find out the other.

If we know the angle through which the horizon has revolved, we know the angle through which the line drawn to the centre has revolved; and, conversely. Now, the angle through which our horizon has revolved, when we have moved from one place to another, we can always tell by *observation*. We have only to observe by how great an angle any fixed *star has apparently* been made to approach the horizon, or recede from it.

For what appears to us to be the approach of the star to the horizon, is in reality nothing more than the approach of the horizon to the star; and what appears to be the elevation of the star above the horizon is the sinking of the horizon below the star. Thus, then, we have only to observe the angle through which (by reason of our motion) the star appears to sink or to rise; and we shall know the angle through which our horizon has really been made to revolve; and, therefore, the angle through which an imaginary line joining continually the place on which we stand, and the centre of the earth has been made to revolve. If, for instance, a star has apparently ascended or descended one degree by reason of our change of position, it is our horizon which has in reality revolved through that degree; and, therefore, the line drawn from our feet to the centre of the earth has, by our motion, been made to revolve through one degree, or the 360th part of a complete revolution. We have therefore manifestly, when we have done this, moved over the 360th part of a whole circumference of the earth. And if the actual *distance* through which we have moved be measured, we shall know what distance is one 360th part of the whole circumference of the earth. It will be found to be very nearly 69 miles and one-tenth, or accurately, 69·08 miles. 69·08 miles, then, is the 360th part of the circumference of the earth, or 69·08 miles taken 360 times is the circumference of the earth. It will thus be found to be 24,869 miles.



Knowing thus the circumference of the earth to be 24,869 miles, the rules of Geometry tell us that its diameter must be 7916 miles.

Thus, then, it has been accurately demonstrated; and, if the reader has followed the argument, he *knows* with as certain a conviction as that which may be obtained from a proposition of Euclid, that this earth on which we stand is a great ball, somewhere about 25,000 miles in girt, and 8000 miles in diameter; so that, vertically downwards 4000 miles beneath us is its centre, to which we might complete a journey, travelling day and night, at the rate of ten miles an hour, in about sixteen days.

We have great difficulty in forming any conception of so huge a mass. The largest object we have an opportunity of observing, and obtaining the same notion of as we usually do of the dimensions of objects, is a mountain. Now, the highest elevation on the earth's surface is not more than five miles in height. If it were instead of five miles two hundred and fifty miles in height, it might bear the same relation to a sector of the mass of the earth, that an object lying in the space beneath the dotted line in the accompanying diagram might do to the whole figure. Being, as it is, only five miles, or one fiftieth of this in height, it is impossible to make a mark on the figure such as could be seen, which would at the same time represent the proportion of the greatest mountain in existence, to one narrow slice or sector of the mass of the earth. The ocean, it is probable, nowhere exceeds five miles in depth; the extreme inequalities of the solid portion of the earth's surface are, therefore, nowhere more than ten miles. This is one-twenty-fifth of the space included between the curved lines in the diagram, a distance which will about be represented by the thickness of the paper on which this is printed.

Thus, then, any irregularities which exist on the surface of the earth are as nothing when compared with the whole mass of it. The greatest mountain is but as a speck of dust on the surface of this globe; the deepest sea but as the irregular and almost imperceptible erosion of its surface, which, if it were of metal, might be produced by the action of the air upon it; and the channel of a mighty river but the scratch which might be made upon it by the slightest pressure of the point of a needle. If this huge mass be, as indeed we know it to be, subjected to the action of central heat, how slight a developement of this would be required to obliterate a continent or upheave the rugged bottom of an ocean. The merest throb, the most imperceptible breathing of the huge mass, the feeblest pulse of its great heart, would be sufficient to account for a complete change in the relative positions of land and water. Such changes we know at different times to have taken place: there is scarcely a single spot on the earth's surface, now dry land, where, if you seek for them, you will not find evidence that it was once at the bottom of an ocean. And considering that the earth, so far as we can examine it, is composed of substances of an infinite variety of different kinds, as to their chemical constitution, and which are subjected, more or less, to the operation of heat, and possibly of intense heat, it is so far from being a wonder that the thin film of hill and dale, land and water, which constitute its surface, should alter its form by reason of the internal action of its component parts, that it seems to be something little short of a miracle which preserves it from day to day at all in the same form, and which restrains the tendency of its materials to rush into one universal conflagration. "Excedit profecto omnia miracula," says Pliny, "ullum diem fuisse quo non cuncta conflagrarent."

Of the internal composition of this huge mass we know nothing *certainly;* it is a noble field, therefore, for speculation, and one by no means lost; the labourers in it are very numerous. To account for volcanoes, which are but as, here and there, and at long intervals of time, the weepings of some pore of the great body of the earth; some of these demand that we should conceive what we stand upon to be but a shell, wherein is enclosed a vast lake of burning matter: this huge caldron, on the scum of which we must be supposed to be dwelling, they take to have its contents perpetually in a state of circulation; and sometimes, by some storm upon the surface, to upheave the crust which covers it in an earthquake; and at others, through some abraded portion, or some crack in it, to let out a volcano. Others, again, tell us that, about the earth's centre, there dwell, in their primeval metallic lustre and purity, unsullied by all touch or tarnish of oxygen, the metallic bases of the alkalies and earths,—the hidden spirits and active principles of which the solid material substances on which we tread are but a gross oxygenated manifestation; that, ever and anon, water breaks in upon this sensitive mass, and then follow that train of epidermatous calamities which we know the earth occasionally to suffer under; for these metals are some of them so exceedingly impatient of the presence of water, that they become convulsed at the approach of it, and its actual contact causes them to burst into a flame: so that, in point of fact, volcanoes are no more than the developements of that impatience of the presence of water, that hydrophobia which possesses the fluid metals occupying the interior of the solid film or shell on which we live.

Such being the hypotheses which have been made with regard to the mass of the earth, its interior construction and substratum, the reader need scarcely be informed that the subject is one on which very little is really known. That little is comprised in our astronomical and geographical knowledge of it, and in that system of geological facts (as distinguished from geological speculations) which have been, of late years, so rapidly accumulating.

It may, perhaps, here be mentioned, as connected with this subject, that the mean density of the earth is ascertained to be about $5\frac{1}{2}$ times that of water, and that this is greatly less than it would be if the masses near the centre, subjected as they are to enormous superincumbent pressures, yielded to those pressures according to the same law that we find them here to yield.

Further, it may be mentioned, that the variation of temperature, at different depths beneath the earth's surface, is ascertained to be an increase of somewhere about 1° of Fahrenheit for every thirty-seven English feet: now, if this law of variation do really *continue* as we descend, it is ascertained that the temperature of boiling water will be acquired at about two miles below the surface, and that of melting iron at about twenty-four miles. At the centre it might be somewhere about 120 times this heat.

The question, however, after all, of the internal structure of the earth, is one which scarcely belongs to astronomy; sufficient is not known of it, indeed, to claim for it a place in any science. The dimensions of the earth and mean density are all that the astronomer troubles himself about; its dimensions, when known, serve him as a scale whereby to measure the distances and dimensions of the other planets of our system, and its density enables him to *weigh* them.

The determination of the positions of places on the earth's surface, and the tracing of the boundaries of land and ocean, constitutes the science of geodesy. As will be shown hereafter, the earth turns perpetually round one of its diameters, producing thereby the alternations of day and night; this diameter is called the *axis* of the earth, and its extremities are the *poles*. A circle drawn midway between the two poles is called the *equator*, and the two equal portions into which this equator divides the earth, are called its northern and southern hemispheres. A great circle* anywhere drawn round the earth, through its two poles, is called a meridian, and being drawn through any particular place, it is called the meridian of that place. The whole meridian being supposed to be divided into 360 equal parts, each of these is a degree, and the number of these degrees between the equator and any place through which that meridian passes, is called the latitude of that place. Each meridian going round the earth passes through the equator, and cuts it at right angles. This equator being divided like the meridian into 360 parts or degrees, the number of these degrees between the meridian of any place and the meridian which passes through the observatory at Greenwich, is called the longitude of the place. It is called east or west longitude according as the degrees are counted eastward or westward from the meridian of Greenwich.

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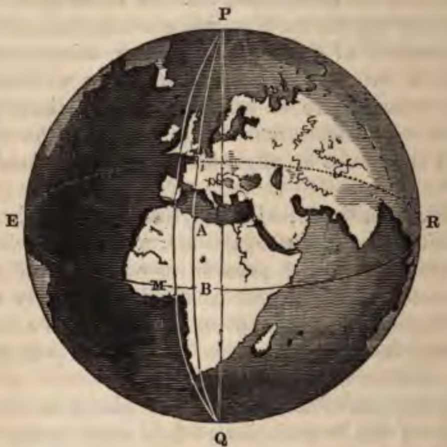
* A circle may be described on a sphere as *small* as we like, but we cannot describe a circle as *large* as we like. The largest circle which we can describe is called a great circle. It is that whose plane goes through the centre of the sphere; or, if we cut the *sphere through its* centre, it is that circle which would bound the section. *We can, manifestly,* describe as many as we like of circles as great as this on the sphere, *but none greater.*

Knowing the latitude and longitude of any place on the earth's surface, we can find out the spot corresponding to it on an artificial globe or on a map; for counting off a number of degrees equal to its longitude on the equator east or west of the meridian of Greenwich, we learn the position of its meridian; and counting off on this meridian a number of degrees from the equator equal to its latitude, we find out whereabout the place is on its meridian. Thus knowing its meridian, and knowing whereabout it is on it, we know the exact position of the place.



Thus, if P be the north pole, and Q the south, a circle ER midway between them is the equator, EPR is the northern, and EQR the southern hemisphere. Also, if A be any place on the earth's surface, then a circle PAQ, going through P, A and Q, is the meridian of A. If the whole circle of this meridian be divided into 360 equal parts, each of them is a degree of latitude, and the number of these degrees between A and B, is the latitude of A; whilst if PMQ be the meridian passing through Greenwich, and the equator ER be similarly divided into 360 degrees, each of them is a degree of longitude, and the number intercepted between B and M is the longitude of A.

On the ocean there are none of those means of ascertaining the exact position of any place where we may be, that are to be found on shore; there are no known objects that we can recognise, there are no beaten tracts with the direction of which we are acquainted, and there is no one of whom we can inquire. But could we ascertain by any means the latitude and longitude of the place we are in at any time, we might easily find out, by reference to the globe or the map, what that place was, and in what direction, or at what distance it lay from the place of our destination ; thus we should know how to shape our course to it, and be able to tell when our voyage would probably end.

The determination of the longitude and latitude by astronomical observation is, therefore, the great problem of nautical astronomy, and with such accuracy is this problem now solved, that ships are frequently months at sea without sight of land, and yet is their course steered continually, and almost without wandering, to some little speck of land, of which they see nothing until they are within a mile or two of it, but towards which for thousands of miles their voyage has been directed through the pathless wilderness of waters.

The following is a method of determining the latitude; we shall point out several others as we proceed.

Suppose an observer to be situated at the equator of the earth; his horizon touching its surface will then be parallel to the earth's axis, and the polar star will just be apparent upon its margin. Let him now move northward; for every degree of the circumference of the earth over which he thus moves, the star will appear to ascend one degree, his horizon rolling from the star one degree. Thus then if he travel on the meridian, the ascent of the star in degrees, or its height above the horizon from which it has ascended, will always equal the number of degrees of the meridian over which he has travelled; but this number of degrees is the latitude. His latitude is, therefore, always equal to the elevation of the polar star above his horizon. Here then we have a very simple and easy method of finding the latitude. We have only to observe with an instrument constructed for that purpose, the number of degrees in the elevation of the polar star above our horizon, and this will always be the latitude of the place where we make our observation. We have here supposed the polar *star* to be accurately in the pole of the heavens, which it is not, it will hereafter be shown how any inaccuracy arising from this cause may be got rid of.

In speaking of the ascent or descent of a star caused by a change in our position on the earth's surface, we have supposed the heavens to be apparently at rest, so that any alteration in the apparent position of the star, must result from our motion, and nothing else. Now, in point of fact, every portion of the heavens except its pole, and every star in it except the polar star*, appears to be incessantly in motion, so that even if we did not move *our* position, *they* would still apparently move, revolving completely round in twenty-four hours.

In ascertaining the variation of latitude by observation upon the apparent motion of a star, it becomes, therefore, necessary to allow only for that motion which arises from the observer's change of place, and reject that which arises from the apparent diurnal motion of the heavens. If, for instance, making an observation on the height of a star here at six in the evening, I travel sixty-nine miles, or one degree northward, and make another observation upon the same star, except I make this observation precisely at the expiration of a particular time from my first observation, I shall find that the star has altered its position much more than one degree,—it will have revolved with the heavens through a considerable space, and this cause much more than my motion will have tended to alter its apparent position. This difficulty may be obviated by the following considerations.

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* The polar star is not accurately at the pole; it turns, therefore, apparently round it, but it is in a very small circle that it revolves, so that it may be considered *nearly* at rest.

Let the plane of the meridian of the observer be supposed to be produced so as to intersect the great vault of the heavens, its intersection will cut out a great *circle* of the heavens, called a celestial meridian. This circle passes through the pole of the heavens and the zenith* of the observer; and since the altitude of a star is measured directly from the horizon to the zenith, when the star is on the celestial meridian its altitude is measured on this circle. Now, let us suppose the altitude of a star to be thus measured when it is on the celestial meridian, and let the observer travel to some other place southward, the star will in the mean time move off the meridian; and if he observe it again when it is so, off the meridian, he will not know what part of the motion which he will observe to have taken place in it is due to his change of position, and what is due to the motion of the heavens. But let him wait until the star comes on the meridian again, and then make his observation, and the result will be precisely the same as though the star had not moved at all during the interval, for it will have returned to precisely the same place on the meridian as it had before. He may therefore suppose it not to have moved. Thus, then, observing the *meridian* altitudes of a star at two different places, the difference of *these*, (that is, the angular ascent or descent of the star,) he knows to equal the difference of latitudes of the places of observation.

A sphere has this property, that all lines drawn from its centre, so as to make equal angles with one another, include equal distances or lengths on its surface between them. Hence, therefore, if the earth be a sphere, any two lines drawn anywhere from its centre to its surface, so as to include the same given angle, say one degree between them, will include also the same length on its surface between them; or the length or distance between these points, measured on the earth's surface, will be the same wherever the two points are taken. The question, whether the earth be a sphere or not, is therefore readily put to the test.

We have only to measure the length corresponding to a degree at different points of the earth's surface. If these lengths be everywhere the same, we know that the earth is a sphere.

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* The zenith of an observer is the point *immediately above his head* in the *heavens.*

The following table contains the lengths, in feet, thus observed to correspond to an angular inclination of the verticals of one degree at different places.

	Feet.		Lat.
Sweden	365782	Svanberg	58°
Russia	365368	Struve	57
England	364971	Roy, Kater	52½
France	364535	Delambre	47
Rome	364262	Boscovich	43
Cape of Good Hope	364713	Lacaille	33
India	363044	Lambton	16
India	363013	Lambton	12½
Peru	362808	Condamine	1½

It will be perceived that these admeasurements range from sixty-six degrees of latitude, or within twenty-four degrees of the pole, to within one degree of the equator, and that they are made not all in one meridian, but in different positions round the earth, and yet they all agree in giving for the length of the portion of its surface, lying between two verticals which contain the same angle of one degree, within five hundred yards of the *same* quantity, viz. sixty-nine miles and one-tenth. Hence, then, it follows, that since lines drawn in different places from its centre, making the same angles with one another, intercept portions of its surface nearly equal in length,—it is very nearly a sphere.

A VISIT TO THE QUICKSILVER MINES OF IDRIA;

IN A LETTER FROM AN OFFICER IN THE AMERICAN NAVY.

YOU know I travelled through Germany as a pedestrian—a mode of travelling which I would recommend to others through that interesting country. You must imagine me, then, on the second day of my journey from Trieste to Vienna, in a region thickly settled and well cultivated, and with a mixture of hill and dale sufficient to make it highly picturesque. An old countryman, with whom I stopped to converse about noon, informed me that by taking a cross-cut over the country, I should make my road to Idria much shorter than by following the high-way, and as I am fond of by-ways I received his information with pleasure, and soon after struck into a wagon-track, to point out which to me, he kindly left his work. The wagon-track, after leading me through some retired villages, dwindled into a foot-path, and even this soon after disappeared and left me alone among the hills: but a lover of nature is never solitary, and particularly with such varied and beautiful scenery as almost every step opened to view. I am strongly tempted to describe some parts of it, and also the simple and hospitable manners of the people—but this would not be exactly suited to a Journal of Science. The country, towards evening, became a constant succession of steep-rounded eminences, generally of considerable height, and just before sunset, reaching the summit of one of the highest, I had just under my feet the pretty little town of Idria. It is situated at the bottom of a deep valley or green, the houses were white, and as the streets have to follow the windings of the green ravines, it has a simple and very pleasing appearance. Near the centre, is a conical hill with a church on its summit, from which a line of a dozen little chapels, along the side of the eminence, showed the course of the Via Dolorosa—sometimes an appendage to papal churches. A stream of water of about forty yards in width, dashing along the bottom of the valley, and several of the excellent German roads, running zig-zag up the steep ascents, completed the view.

At the entrance of the village my passports were examined, and the officer, having ascertained that I wished to examine the mines, said he would send a person to accompany me. Accordingly, a serjeant soon after called at the public-house were I lodged, to say that the mining operations were carried on day and night, and that I could enter at any time: I had noticed from the hills a dark crowd of men in front of a large building, and those, he told me, were the evening gang about commencing the descent. I appointed six o'clock in the morning, and on waking, found him waiting for me. At the building alluded to, which is on one side of the village, and covers the entrance to the mines, we changed our dresses, and the keeper unlocking an iron gate, we found ourselves in a horizontal gallery three or four hundred yards in length, running directly into the hill at the foot of which the edifice is erected. Here we came to a small chapel with a light burning before the picture of the Virgin, and turning short to the left commenced the descent. It has nothing difficult, being effected the whole way by means of stairs in pretty good order: indeed, the mines have nothing corresponding to the ideas of terror which we are apt to connect with such places, except the atmosphere, which, throughout the mine, must be strongly impregnated with mercurial vapour, and is constantly producing salivation among the workmen.

Having descended by seven hundred and twenty-seven steps, reaching to a depth of one hundred and twenty-five fathoms, we arrived at the region where chiefly the cinnabar is procured. The mining-operations are carried on principally in galleries, the friable nature of the ground or rock seldom admitting of larger chambers. The cinnabar is in strata of from two to six inches in thickness, and of a variety of colours from dark to light red, the quicksilver sometimes being mixed with it, sometimes occuring in the intervening strata of earth or stone. Sometimes the cinnabar is of a brilliant red, and once I found it in small crystals, but such specimens are rare: generally it is of a dull-red colour, and the stone is so brittle that nothing more than a pick-axe is required. The strata affording the quicksilver appeared to have no particular direction, and occupy about one third or one half of the entire mass of rock. Proceeding a short distance, however, we came to galleries where the cinnabar is less common, and the quicksilver is the chief object of search. It occurs here sometimes imbedded in a friable rock, sometimes in a *kind of* earth, in appearance and hardness resembling talcose slate, but *principally* in the former. Generally, it is in particles too minute for *the naked* eye; but often, when the rock is broken, small globules present themselves, varying from a size just large enough to be seen up to that of a common pin's head. These globules are not distributed at random through the mass, but the substance in which they occur forms strata usually about one inch or two in thickness.

Descending still lower, we soon came to the richest part of the mine. Here the *gangue* consists almost entirely of talcose earth mentioned above, and the globules are so large that when it is broken, they fall out and roll to the bottom of the gallery. The labourers here are relieved every four hours, being unable, from the state of the atmosphere, to work longer than this at one time. In the other parts of the mine they work eight hours. There are three hundred and sixty altogether employed in the mines, divided into three companies, and working, each, eight hours out of the twenty-four; their pay is only from 15 to 17 kreutzers (5*d.* to 6*d.* English) a day, the usual pay of day-labourers throughout Germany. I found several of them suffering from the effects of the mercury.

Having loaded myself and the guide with specimens, I returned by the same way to the upper mine, and proceeded next to examine the washing-rooms, which are situated a few hundred yards from the mines. The *gangue* containing the metal is carried to this house, and if it is of the earthy kind, it is broken up and thrown upon large sieves, by means of which the loose or native quicksilver (called here *jung frau*, or virgin quicksilver) is separated from the earth: the latter is then cast into shallow boxes, open at the ends and a little inclined, and a gentle stream of water being made to pass over it, a rake is used, and the earthy matter is carried off. There are seven of these boxes in succession, and by the time the residuum reaches the last of them, it resembles a heavy gray powder, and is sufficiently pure to be carried to the vapour-furnace. The stony fragments require only a slight washing to cleanse them from the outward earthy impurities.

The furnace is half a mile lower down the valley, and at the extreme end of the village; it consists of a circular walled building about forty feet diameter by sixty in height, on each side of which is a continuous range of chambers ten or twelve feet square, and nearly as many in height: by means of small square openings in the partition walls, the air is allowed to pass from the centre building to the remotest. Each has also a door communicating with the external air. These buildings are all of stone and are plastered within. The *gangue*, after being prepared in the washing-house as already described, is removed to this edifice, and placed in earthen pans four inches deep and fifteen in diameter, which are piled up so as to fill the centre building. The doors of the chambers are then carefully walled up; and a strong fire having been lighted under the centre building, the quicksilver rises in the form of vapour, and passing into the small chambers, is there condensed by the cold atmosphere around them. Some of the gangue, you will observe, is brought here in the form of the native rock: I understood them to say that the expansive power of the vapour, together with the heat of the fire, was sufficient to cause the rock to disintegrate, and thus allow the escape of the quicksilver. When this process is over, the doorways of the chambers are once more opened, and the quicksilver, which is found chiefly adhering in drops to the sides and ceiling, is scraped off, and, running into a hollow in the floor, is taken thence to the cleaning and bottling room. It appears to act on the mortar of the chambers, for I found the latter flaky, and the crevices all filled with small globules.

The cleaning-process is very simple, a piece of canvass being merely spead over a funnel, and the quicksilver, being made to pass through this, comes out sufficiently pure. That intended for home consumption is then tied up in sheepskins, while that for exportation is put in iron bottles large enough to contain sixty-eight pounds. The furnace is kept in operation only during the winter months, and then the vapour which escapes from it is a serious annoyance to the town: they have a blast three times every fortnight.

The price of quicksilver at the mines is 112 florins for one hundred German pounds. The quantity annually procured is about one hundred and sixty-four tons: formerly it was greater, and brought a better price, their market, which is chiefly in China, having been injured by competition from the quicksilver mines, near Almeria, in Spain.

[From SILLIMAN'S *Journal.*]

HORÆ MAGNETICÆ.—TERRESTRIAL MAGNETISM.

II.

[The original intention of the author of these papers was, to give a complete history of the efforts which had been made to solve the great problem of terrestrial magnetism, and to point out the merits and defects of each, and to lay before his readers the exact state of each branch of the inquiry; but circumstances have arisen to change that plan; and he is compelled, therefore, to close it with the present section, in which, however, the chief researches concerning it as a *mathematical problem* have been more or less dwelt upon, so as to enable the young inquirer to perceive what is his starting-point, and the places to which he must look for further details.

With respect to the thermo-magnetic and the electro-magnetic theories of terrestrial magnetism, as they are of a more popular character than the purely mathematical, as derived from an assumed law of force, or the empirical mathematical, as derived from a comparison of observations, we may be led in a future Number to speak of them as so many corollaries from the developed principles of those sciences which it is our intention hereafter to give in a familiar form.

The researches of Poisson, and other writers of the same class, have so remote a bearing upon the actual case of the earth, that we have not felt it at all necessary to give any account of them in a popular sketch like the present; nor, indeed, strictly speaking, do they admit of it distinctly in any other than the symbolic language of technical mathematics.]

The observed phenomenon, that when a magnetic needle was changed from its position of rest in any given place by an external force, as soon as that force was removed, the needle, acting under the influence of the forces which gave it the directive tendency, passed back to its original position with a continually-increasing velocity, and was then carried *almost* as far to the other side of its position of final repose —then returning in the same manner, and again passing though its position of repose towards the position into which it had been originally moved, but still not *quite so far* as it had passed on the other side:— the series of oscillations which it made in this manner, till it finally came to rest in the natural position of repose at that place, led, very naturally, to a comparison between its motions and that of a pendulum acted on by gravity. It was easily inferred, that, whatever were the nature of the forces which acted upon the needle, they were equivalent to a single force, or resultant, which acted in that particular direction, and that the laws which governed the motions of a common pendulum must also govern the motions of a magnetized needle; and, therefore, that the intensity of the force at any given time and place may be compared, as to ratio, with the intensity at any other time and place, by the number of oscillations which the *same* needle would make in any given specified time—as, for instance, in ten minutes; or, again, by comparing the times in which, under those varied circumstances, the needle would perform some specified number of vibrations; as, for instance, one hundred, two hundred, or three hundred. Various modifications of the principle have been employed, such as the time required for reducing the arc of vibration from some given number of degrees to some other smaller number of degrees. The relations between the several methods are easily deduced in the form of equations by methods familiar to *those who understand* the elementary principle of forces, but which could not be rendered intelligible to those who do not. It is, consequently, unnecessary to dwell upon them here.

Long before Humboldt performed his experiments on his American journey, it had been suspected that the intensity of the directive force increased as we recede from the geographical equator on each side; but these experiments showed that such was at least *approximately* the case, and though there are great uncertainties attendant upon all such experiments, there was yet such an accordance between the calculated results of the observations, as to leave no doubt of its general truth. The line, however, at which the intensity is a minimum, does not coincide with the geographical equator, nor the points at which it is greatest with the geographical poles. The researches of Morlet, to which we referred in our last Number (p. 229), have reference to the line at which the needle takes a horizontal position: those of Hanstein have been directed to the determination of the line at which the *horizontal* intensity of the needle is constantly the same—that is, to the line along which it is the least, the points at which it is the greatest, and the lines at which it is any given quantity intermediate between these. We shall make a few remarks on each of these processes, and upon the principle employed in them.

From what we have already said*, it is obvious that the method of Morlet is defective in this—that his *magnetic meridians* are not in any way connected with the phenomena themselves. They have no definite relation to the position of the poles, except those poles be situated in a single molecule at the centre of the earth. Now Biot's researches, to which he especially refers†, do not, certainly, justify this assumption; for they only show that the hypothesis of the magnetic axis passing through the *centre* of the earth more nearly accords with the conclusion of the poles being situated in a single molecule, than with their being at a finite distance from each other; but instead of justifying this inference, his results only tell us that two poles, situated in the same terrestrial diameter, and at equal distances from the centre, do *not* fulfil the conditions nor account for the phenomena. This we say, referring to the *dip only;* and for the variation, it furnishes results inconsistent in character as well as in quantity, which latter circumstance is of itself fatal to the whole hypothesis, independent of any other consideration.

Biot's method‡, to which we have referred in our last Number, at p. 225, is to express the resultant of forces acting on the branches of the needle (in the usual manner of treating the action of central forces on a lever whose fulcrum is fixed), by means of an equation between rectangular co-ordinates, the co-ordinates being referred to the centre of the earth as origin, and the magnetic axis as the axis of one of them. On his hypothesis, this involves the *arbitrary* quantity, a, the semi-length of the terestrial magnetic axis. He then transforms his equation into a polar one, in terms of r and u; and finally, by very simple steps, obtains the expression,

$$\tan \beta = \frac{\sin u}{\cos u - \mathrm{K}\left\{\frac{\mathrm{D}'^{3} + \mathrm{D}^{3}}{\mathrm{D}'^{3} - \mathrm{D}^{3}}\right\}}.$$

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* *Journal of Popular Science*, No. IV.

† *Mém. des Savans Etrangers*, tom. iii.

‡ *Philos. Mag.*, vol. xxii., p. 300.

In which β denotes the inclination of the dipping-needle in its natural state at the place of observation to the magnetic axis: u, the great-circle distance of the place of observation from the point at which the magnetic axis cuts the surface of the earth: D' and D the distances of the centres of magnetic force from the place of observation; and $K = \frac{a}{r}$, r being the terrestrial radius.

He then performs the calculations for the observation made by Humboldt at Carichana, in N. lat. 6° 34′ 5″, and W. long. (from Paris) 70° 18′, supposing K to have different values, from unity downwards to zero, and compares them with the actual dip obtained by Humboldt. The following is his table of results; the observed dip being 33·78° of the centigrade division of the quadrant.

Values of K.	Dip of the Needle.	Errors.
$K = 1$	7·73° Centigr.	26·04°
$K = 0{\cdot}6$	18·80 "	14·97
$K = 0{\cdot}5$	22·04 "	11·73
$K = 0{\cdot}2$	29·38 "	4·39
$K = 0{\cdot}1$	30·64 "	3·13
$K = 0{\cdot}01$	31·04 "	2·73
$K = 0{\cdot}001$	31·07 "	2·71
$K = 0\cdot$	31·0843 "	2·69

In the first of these the poles are taken on the surface, and in the last at the centre, or coincident in a single molecule; and it was inferred by Biot, that, because the errors continually diminish as we approach the earth's centre, the nearer to that centre we take the centres of force the more accurately we shall represent the phenomenon. Still, as even in the *extreme* case there is a relatively considerable error, the just inference would have been, that, with no relation between r and a, having the poles fixed in the *same diameter*, and at equal distances from its centre, could the phenomenon in question have been represented. The errors, it must be remembered, too, are *all on one side*, which is further condemnatory of the hypothesis itself. This, added to the other inevitable consequences noticed at page 228, ought to have long ago banished this hypothesis from our standard works on magnetism; and certainly would have done so, had a proper spirit in philosophy existed amongst us.

Fallacious, however, as this hypothesis is, it is the basis of nearly everything that has been done in the way of calculation in terrestrial magnetism. This we proceed to show.

In Biot's formula, when $K = 0$, we obtain the expression $\frac{\sin u}{\cos u - \frac{0}{0}}$, in which the second term of the denominator is apparently indeterminate: but by recurring to the usual methods, known to every mathematician, for ascertaining whether such be the case, and its value when not, it is found to designate $\frac{1}{3 \cos u}$, and the value of the expression is then $\tan\beta = \frac{3 \sin 2u}{3 \cos 2u + 1}$.*

By a little easy management, the formula of Kramp, noticed at the close of our last article on this subject, is obtained; *viz.*—

tan. inclination = 2 tan. magnetic latitude†.

And it is by means of this that Morlet determines the position of points in the magnetic equator, of which we proceed to give a detailed account.

Like most preceding writers he defines the magnetic meridian as "une grande cercle de la sphère terrestre, déterminé par la plan verticale qui est mené par la résultante des forces magnétiques au lieu de l'observation‡." This is the principle, indeed, on which the preceding investigations of Biot, as well as those of Kramp, were founded. On the meridian thus determined, he finds an arc to fulfil this condition, and its extremities he takes for a point in the magnetic equator. Through an assemblage of such points he describes a figure, so inflected between the points, as to form one continuous line. The process is sufficiently simple, and needs little further explanation: but this memoir is a remarkable specimen of the affectation so common amongst mathematicians—the application of numerical computation and algebraical formulæ, which profess great accuracy, to inquiries where, from the very nature of the means employed in experimenting, the data of observation must necessarily be very uncertain. He applies the differential equations of maxima to ascertain the utmost limits of error§; and that, too, to a formula so inaccurate as we have seen Kraft's *necessarily* is, and on a subject where the uncertainties of the truth of the observed data amount, under the most favourable cases, to a *whole degree*. What is the use of investigation of formulæ for the most favourable cases, where the difference between the most favourable and the least favourable can never amount to more than a small fraction of that essentially uncertain quantity? It is, without doubt, one of the most dangerous errors in every science—but in none so much, perhaps, as in terrestrial magnetism—to attempt to allow mathematical refinement to usurp the place of careful experiment; and it would not be difficult to point out many instances of the injury which physical inquiry has sustained from the too-prevalent reliance upon the supposed power of mathematical investigations, to alleviate the toil of inductive research‖. Before the present age, it will

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[*] Biot uses the expression in its complex form, $\tan\beta = \frac{\sin 2u}{\cos 2u + \frac{1}{3}}$, and so do most other writers; but the form in the text is certainly better fitted for *actual computation*.

[†] Biot, *Traité de Physique*, tom. iii. p. 133.

[‡] *Mém. des Savans Etrangers*, tom. iii. p. 135. The reference to vol. iv. in our last, was an oversight in transcription.

[§] *Mém. des Savans Etrangers*, tom. iii. p. 136.

[‖] We would not be misunderstood here. In every computation, founded on the mean of many observations, the mean result should be taken to small quantities, as to seconds in time or arcs, and to three decimals at least in linear measure, even though the observations themselves were taken only to much larger intervals. Moreover, in the investigation of formulæ, we should always hold that the most favourable and the most unfavourable cases should be shown in order that observers should, as far as *possible*, select the one and avoid the other; and that the calculator should also, *where there* is room for selection, choose in the same way. It is also desirable, when be admitted by every one acquainted with the subject, that, with the extreme difficulty of making unexceptionable observations, there are exceedingly few recorded ones upon which the confidence necessary for such determinations can be placed: and even of those made more recently, those philosophers considered best qualified to judge, believe that very few are fitted for such a determination as that which Morlet undertakes, even could we admit the formula by which he performs it. Of the dip, we believe most persons have a notion that its determination is a very difficult process; and those who know anything of magnetism as a science, are fully aware of the uncertainty of the results when made with the greatest care and most complete instruments; and we have heard it affirmed by one of the most distinguished and cautious philosophers now living, that he has no confidence even in the variation (which seems, by common consent, to be admitted as a perfectly determinable quantity) to the amount of a *whole degree*. When, therefore, we reflect upon the necessary inconveniencies under which such observations are made near the equator, at the several places where they are recorded—many of them even at sea, and others in volcanic regions, in which considerable local disturbance must exist—with instruments of different degrees of accuracy, and by observers of very different degrees of skill,—when we reflect on these, and other sources of error, we must be fully convinced of the utter inadequacy of the collected data to furnish even an *approximate* determination of the actual course of the magnetic equator.

The paper of Morlet is one of considerable labour, and great ingenuity is displayed in several parts of it. The exceptions we take to it are not either to the skill or the perseverance, but, as we have already stated, to the formula he has used, and to the essential imperfection of his data. We are not, indeed, so sanguine as to hope that this latter desideratum will be soon supplied, even if it ever should be; though we look with some degree of confidence to the requisite mathematical formulæ being developed at an early period.

Looking to the circumstance, which is considerably better established, that the lines of equal dip are not circles, we think the observations that have been made near the magnetic equator are fully borne out in their indication of that equator not lying in one plane, or, in other words, not being either a great or less circle of the sphere. Morlet's determination of the remarkable points in it are annexed, and by marking these on a globe or map, the reader will form some general idea of its course. For a more detailed one we must refer our readers to the chart which accompanies that paper itself.

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this selection cannot be made, to be able to compute the utmost probable amount of error attendant upon each datum so supplied.

In the present case, however, the utmost errors that could arise must be far less than those which are involved in the *formula itself*, and far less too than are necessarily attendant upon the methods and circumstances of the observations, even when made with the utmost care, and, what is more, with the utmost good faith. The application of such methods to any of the observations yet made, may be justly, all things considered, stigmatized as an abuse of mathematics; and the same may be said of a great number of showy applications of an analogous kind in other branches of *physical science.*

	Lat.	Long.	Date.
Maximum south lat.	13° 59′ S.	20° to 30° W.	1776
	15 35 S.	40 to 50 W.	1822
Minimum south lat.	00 00 S.	120 to 130 W.	1795
	2 16 S.	120 to 130 W.	1823
Maximum north lat.	3 30 S.	about 165 W.	1778
Node with the terrestrial equator	0 0 S.	about 180 E.	1779
	0 0 S.	174 55′ E.	1825
Maximum north lat.	11 43 N.	50 to 67° E.	1776
Node	0 0 N.	about 17 E.	1780
	0 0 N.	3 45 E.	1822

In comparing the true lines determined for the epochs of 1776 and 1825, as determined by his calculations, M. Morlet is led to the following conclusion.

"Si la courbe sans inclinaison, tout en conservant toujours la même forme, était transportée parallèlement à l'équateur par un mouvement commun à tous ses points, les sommets de courbe (*maxima* et *minima* de latitude) et les arcs parallèles à l'équateur n'éprouveraient qu'un simple mouvement en longitude; et leur latitude resterait invariable."

How far even his own table bears out this conclusion, we leave our readers to decide.

To exemplify still further the insufficiency of this formula, we may refer to Mr. Barlow's calculation of the position of the pole to which the several magnetic meridians (the preceding definition of it) should converge.* This is in itself, if other proof were wanting, a demonstration of the inapplicability of Biot's conclusions, to the actual phenomena of terrestrial magnetism: and we think it the more important to notice these, as this method is to the present hour the one generally employed in calculations relative to these subjects.

On these results he remarks, "that although from the determinations relative to the dip and variation of the needle, we cannot expect the most perfect accuracy, yet it is very obvious, from the preceding table, that the aberrations in latitude and longitude of the magnetic poles are much greater than can be attributed to errors of observation. It will be seen that the place assigned to it differs in longitude as much as 57° between one set of observations and another, and as much as 14° in latitude. It will be also observed, that as we approach the north and west, the more westerly we find the place of the pole, and the more easterly the place of observation, the greater is its latitude. In short, it is evident, from the few examples we have taken, that every place has its *particular polarizing axis*, which probably in all cases fall within the arctic circle, that this is the narrowest limits we are able to assign†." The same discrepancies are also observable in his calculations of the observations of Sir John Franklin, all made, it will be observed, with the same instruments, and by the same observer, and consequently more strongly indicative of the fundamental error of Biot's hypothesis.

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* *Essay on Magnetic Attractions*, 2nd Edition, p. 207; or *Encyclo. Metrop.*, *Mixed Sciences*, vol. i., p. 815.

† *Idem, p. 208,* aliter 816, col. i.

Place.		Date.	Lat. of N. Mag Pole		Long of N. Mag. Pole.		Observer.
			°	'	°	'	
Tristan da Acunha		1821	70	56	49	53	Captain Marryat
Trinidad		do.	73	59	47	20	Do.
St. Jago		1820	69	37	67	4	Mean of Capt. Marryatt's and Lieut. Mudge's observ.
Teneriffe		do.	69	49	69	14	
Madeira		do.	68	4	65	26	
Madrid		1799	72	47	50	33	Humboldt
Paris		1814	75	31	67	4	Bouvard
London		1818	75	2	67	41	Kater
Berlin		1805	79	2	70	44	Humboldt
Copenhagen		1813	79	43	67	38	Wluegel
Davis's Straits		1820	67	37	94	26	Sir Edw. Parry
Regent's Inlet		do.	71	10	98	16	
Baffin's Bay		do.	71	13	97	3	
Possession Bay		do.	69	40	09	10	
Melville Island		do.	73	12	102	46	
Stations in North America.	1	1821 and 1822	65	11	100	5	Sir John Franklin
	2		64	47	102	14	
	3		67	35	104	25	
	4		68	17	104	24	
	5		70	17	106	21	
	6		69	51	107	31	
	7		68	58	105	54	
	8		68	50	107	33	

This is, we think, enough to satisfy any candid mind that the problem requires a totally new investigation.

We ought not, however, to pass by the concluding observation of Mr. Barlow, without one remark. We do not, indeed, fully catch the import of that sentence, except it be intended to affirm that there is a different pair of poles (centres of force), to which the needle, at each place of observation, is subjected. If this be the meaning, we are sure he must long since have discovered the total incompatibility between such a doctrine and the fundamental principle of the duality of the centres of force, situated in a central molecule: and had it not been repeated in his *Treatise on Magnetism*, published several years after his *Essay*, we should be led to think it a mere slip of the pen. That the mode of action, the intensity and direction of the resultant force at each different place in his or any collected list of observations should be different, is obviously a necessary consequence of the assumed circumstances, upon which the formula itself is founded; but that this difference should result from each place having its own "particular polarizing axis," would inevitably lead to the conclusion that the number of poles, instead of being two, is infinite; and still more strangely, that instead of each pair of poles acting in all directions, they have no influence whatever upon any point in space save the particular one to which they are adapted*.

The truth is, that in all these inquiries, two fundamental mistakes have been committed; but of these we shall speak hereafter, merely indicating in this place, that we are prepared to exhibit the source of the fallacy. We reserve them to a future page, because they are connected with some more recent investigations to which we shall then have occasion to advert.

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* It would be curious to inquire whether, at any two places on the same "magnetic meridian," observations have been made, and to ascertain the positions of the poles of convergence by this method; but we are not aware whether two such observations are recorded or not.

From the opening remarks of this section of the *Horæ Magneticæ*, it will appear that it was our intention to speak also here of the curves of equal intensity,—or at least of the curve of least intensity, or of the magnetic equator, according to Hansteen's definition of it. Now, whether the definition of this equator, by means of the position of the needle with respect to the horizon, or by means of the amount of intensity be adopted, we are not left at liberty to assume them as *identical*. For anything that yet appears to the contrary, they may be very remote from each other in actual position, and they may, as mathematical curves, be essentially distinct from one another. At all events, as we remarked with respect to the magnetic poles, if the two *definitions* be of a line which is mathematically the same, that identity ought to be shown by deriving their equations either the one from the other, or the equations of both from some third and different definition. Of course that of the centres of force would be both the most direct and most satisfactory. This has not been done, and hence, for the present, we are under the necessity of considering them as distinct but totally arbitrary modes of definition.

The intensity of a magnetic needle is materially altered by its temperature; and an approximate degree of connexion has been discovered, after numerous and very careful experiments, made by Mr. Christie, and M. Küppfer[*] of Casan, and shown to be a very important element in the determination. All the experiments and observations, however, upon which Hansteen founded his experimental lines of equal intensity were made prior to that period, and hence, from the neglect of temperature, these must necessarily be very inaccurate. The difficulty, too, of making unexceptionable experiments on the intensity, is even greater than in those on the dip and variation.

The great and unaccountable oversight in Hansteen's *method* is,—that he does not seek the lines of equal intensity in the natural direction of the needle, at the several places of observation, but the lines of *equal horizontal intensity*. The intensity, in this case, is not the whole intensity of the magnetic force at the place, but the intensity multiplied by the sine of the dip. What information, then, can this furnish of the nature of the distribution of the magnetic energy at the surface of the earth? The introduction of this foreign element into the investigation, is altogether *injurious* too, inasmuch as it renders the question much more complicated than it naturally is, and is such as to offer no hope of a successful termination to the inquiry. His isodynamic lines (the term which he gives to the lines of equal intensity) are, therefore, productive of no advantage, but of much real injury to the problem itself.

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[*] *Phil. Trans.*, 1824; *Annales de Chim. et Phys.*, 1825. Mr. Christie's were undertaken and completed some time before M. Küppfer's, but there is no doubt of the latter being altogether independent in his views an dresearches. We would not be supposed to say that the influence of temperature on the magnetic intensity was then first observed. It had been known long before, and had been employed by Canton in explanation of the phenomenon of the daily variation, first observed by Graham nearly forty years before. The first experiments, on the influence of high temperatures on the intensity of magnetism, were certainly made by Mr. Barlow, about 1820; and the first efficient inquiries into the *law* of that influence, by Mr. Christie, to whose valuable investigations, both mathematical and experimental, on that subject, we are indebted for nearly all that is known relative to it at the present time.

The same objection applies equally to the points of *greatest* intensity, determined in Hansteen's manner, as to the loci of the points of equal intensity just spoken of. Considering these as the centres of force, whatever be the law assumed, it is easy to show that the phenomena, neither of dip, variation, nor intensity, can be even approximately accounted for; even though we admit the existence of the four such points for which he contends, and allow them any ratio of intensity; but this is not the appropriate place for such discussions. At some future time we may have occasion to return to the inquiry. Nor, again, is it possible to account for, from his *mode* of determination, any one particular relative to the state and position, or to the number of centres of force which produce the horizontal intensities. There is an essential element, the dip, required to render any one of them serviceable. If, however, the real intensities, in the *direction of the resultant* magnetic forces at each place, were mapped out from authentic observations, and due regard paid to the influence of temperature,—such charts would be of inestimable value in the furtherance of magnetic science. Is it too much to hope that some ardent inquirer may undertake such a task? And especially when the subject is one of sufficient importance to ensure due honour to the efforts he may successfully make?

Of the more recent researches of Hansteen, scattered through various continental journals, but especially in Professor Schumacher's *Astronomische Nachrichten*, it is difficult to give any popular idea, further than this,—that they are almost entirely devoted to a determination of the isodynamic lines in different parts of the globe. Of his *Magnetismus der Erde* we may possibly, in some future Number, give a pretty complete analysis, and accompany it by allusion to other matters and other authors intimately connected with the subjects of that work. The above brief account of his labours may suffice for our present purpose.

Of all English writers on the subject of magnetism, and especially on terrestrial magnetism, Professor Robison, of Edinburgh, was undoubtedly the most philosophical and the most original that had appeared from the time of Gilbert till a very recent period. His two treatises*, first published in the Supplement to the *Encyclopædia Britannica*, on "Magnetism" and the "Variation of the Compass," are fine specimens of careful induction, richness of invention, and luminous developement of principles: and we are not acquainted with one single work in the English language, the style of which is calculated to produce a better effect upon the mental habits of the young inquirer, than the writings of this distinguished philosopher furnish us with. They ought to be in the hands of every one whose attention is devoted to any of the branches of physical science on which he treats. We do not, however, in this analysis, intend to speak at much length of his labours, on account of their abrupt

* These are published entire in Sir David Brewster's edition of his works, under *the title of A System of Mechanical Philosophy*, vol. iv., occupying from p. 205 to p. 375.

termination already alluded to, but advert to them chiefly on account of the *magnetic curve*, which was first imagined by him, and which has subsequently been brought to bear on the present inquiry in a way which we shall presently describe.

He first considers the action of the four poles of two magnets, placed in one straight line, and deduces several of the properties of the curve, in which a needle being placed, the direction of the resultant forces should fulfil given conditions*, and thereby explained several phenomena exhibited by two magnets (and also by a single magnet) which had before been deemed inexplicable. He then considers the action of a magnet on iron-filings, and the curves which they form when placed on a paper above a magnet†. In an appendix‡, he inserts an investigation of the equation of these curves for different laws of force, and deduces the remarkable property, that in the case of the inverse square of the distance, the sum of the cosines of the angles (with their own appropriate signs), with lines drawn from any point in the curve of filings to the poles of the magnet, was a constant quantity for each individual curve. These curves are such, that the magnetic axis of the particle of iron is a tangent to the curve at the point in which it is situated. At a period (as we are told by his editor) considerably anterior to this, he had, in his treatise on the variation of the compass, conceived the resemblance between this phenomenon and the action of the great terrestrial magnet upon a small compass-needle§; and he appeared to see, that if we could determine the intersections of these curves with the circles made on the globe of the earth by their planes, so that the needle should have the dip and variation which should conform to experience, the problem would be completely solved. He did not, however, enter upon the inquiry under this aspect*: nor does it appear, from any of his writings, so far as we know, that he had any distinct idea of the mathematical methods by which this may be accomplished.

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* *Mech. Phil.*, vol. iv., p. 221, &c. † *Ib.* p. 269—72. ‡ *Ib.* p. 350—3.

§ Mr. Davies, of whose researches on this subject we shall presently give a brief account, thus defends the propriety of this analogy.

"It will be remarked, that in the equation of the magnetic curve, the final position of the needle, or that which it takes when its centre is coincident with the centre of force, is different from that which is exhibited by a needle acted on by an artificial magnet in all our experiments. This might, at first sight, seem to throw some doubt on the validity of the principle employed in deriving the equation of the magnetic curve; but a little reflection will convince us, that the conditions of such experiments are different from those which obtain in the case before us.

"In all our experiments, the length of the needle itself bears a finite ratio to the distance of its centre from the poles of the magnet upon which we experiment, and hence the action becomes mutual; the two magnets, and the system of action, being thus rendered compound, its results must be expressed by a more complicated formula than in the case we have supposed, and from which our equations of the magnetic curve have been derived. This complexity of the conditions implies a corresponding complexity of the equation by which they are expressed; but uniform experience has shown that as we diminish the length of the needle, and increase its distance from the poles, the observed results are more nearly approximative to the results of the hypothesis from which we have started. In the case of the curves exhibited by iron-filings strewed on a paper above a bar-magnet, the approach of the observed curve to the calculated one is very close. But in this case the length of the needle (or magnetized particle of the iron) is very small in comparison with the magnet, and with its distance from the magnetic poles; and, moreover, has, in itself, so little magnetic intensity as to exert an insensible reciprocal influence on the state of the bar itself. This is precisely a miniature representation of the case of a small needle, acted on by the terrestrial magnet, though the ratio of the particle of iron to the magnetic bar is many times greater than that of a needle to the terrestrial magnet; and hence the discrepancy between the observed and calculated result in the former case is many times greater than in the latter. No ground of exception to our plan of inquiry can therefore be found in this circumstance, but rather a confirmation of its validity, in reference to the use we have made of it." (*Phil. Trans.*, 1836, p. 126.)

More recently, Mr. Davies has undertaken the examination of the mathematical consequences of the theory of *two centres of force*, situated anywhere within or upon the surface of the earth, viewed as the sole or principal causes of the phenomena of dip, variation, and intensity: but as only a very small portion of these have yet been made public, we are unable to give any developed account of his methods or results, further than can be gathered from two papers in the Philosophical Transactions†, one of which only has been actually published, though the other has been privately distributed by the author, and a copy lies before us, of which, together with the Society's Reports, we shall avail ourselves in this place.

His first step was founded on the consideration, that the needle when left at liberty to take a position in the direction of the resultant of the forces acting at the place in which it was situated, would, being prolonged, cut the magnetic axis in some point, or be parallel to it; if, then, from the observed dip and variation, together with the latitude and longitude of the place of observation, the equations of the line in which the needle is situated be formed (to which the above data are necessary and sufficient); and this be done for four different observations, the *requisite conditions for the position of the magnetic axis itself* will be found. It is well known to those versed in this branch of geometrical inquiry, that when four straight lines are any how given in space, so that no two of them be in the same plane, there can be only two straight lines drawn which shall cut them all four. But as the magnetic axis cuts the four lines of direction of the needle, and these four lines have been given by observation and subsequent calculation, there are only two positions of the magnetic axis which correspond to these positions of the needles possible. Moreover, if this inquiry were instituted respecting several sets of needles, and the equations of the magnetic axis calculated from each of them, then if the observations be correct, and the whole phenomena due to the action of two poles (or of any number, and of any arbitrary intensity situated in the same straight line, for the above conclusion corresponds to this hypothesis also), *they ought to indicate the same line.* We have said line, singly, for if we suppose both the lines given in such case to furnish solutions, which they would algebraically, yet it would still only indicate two different positions in *either* of which the magnetic energies *may* be situated *separately* to fulfil the conditions, but not in both simultaneously, as in this latter case we should infer the *actual co-existence* of at least four poles not situated in the same straight line, contrary to the original hypothesis. He, however, proceeds to the examination of this part of the question by another and a shorter process: viz., by inquiring whether either of these axes so determined from four given positions of the needle, is cut by a line drawn in the direction of a fifth needle determined in the same manner. He, therefore, calculated the equations of six different needles, viz., those at Port Bowen, Boat Island, Chamisso, Paris, Valparaiso and Paramatta: and from the first, third, fifth and sixth, the equations of the magnetic axes. In comparing each of these two positions with that of the Paris needle, it is found that the least distance between this magnetic axis and the needle in question is between a sixth and a fifth part of the terrestrial radius—a distance too great to be supposed capable of resulting from mere errors of observation, if the methods of observing be themselves to be trusted: since he states from every other case he has tried, the discrepancies are as great or greater than in the one he has given.

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* *Mech. Phil.*, vol. iv., p. 294.

† "Geometrical Investigations concerning the Phenomena of Terrestrial Magnetism. (*Phil. Trans.*, 1835, p. 221, and 1836, p. 95.)

The very great labour attendant on these calculations offers little inducement, in the absence of data upon which perfect reliance can be placed, to examine them more minutely or extendedly: and hence, though the method in respect to its mathematical processes is complete and simple, Mr. Davies abandons this mode of inquiry, at least, for the present. He, however, remarks, that to lessen the actual labour of a first approximation, he has had recourse to constructions by means of the descriptive geometry; and that "there is a greater degree of approximation in the few magnetic axes which he has thus determined from existing data than appears compatible with any other theory of the constitution of the terrestrial magnet than that which considers the magnetic forces situated in two isolated centres or poles." Still, "he would not be understood to say that the approximation is close, but simply that in comparison with all the positions which the lines may take, there seems to be *one region* in space, in reference to the co-ordinate planes or planes of projection into which they dispose themselves, but dispose themselves very irregularly in it*."

He next proceeds to inquire whether the *general mathematical character* of the phenomena which would result from the hypothesis of two centres of force, situated anywhere within, or upon the earth, are in their larger features correspondent with those which observations seem to point out in nature: to effect which he returns to the investigation of the properties of the magnetic curve of Professor Robison, which he gives for any ratio of the intensity of the two forces, in a form of which the equation of Professor Playfair is a particular case; though in his subsequent investigations he takes the intensities equal, inasmuch as he finds by the application of the natural interpretations of the resulting formulæ, that their inequality would *necessarily imply* the existence of *two* magnetic equators, or lines at any point of which the dipping needle would take a horizontal position†. By changing the method of mathematical investigation, he then finds, admitting the equality of intensity in the two poles for the foregoing reason, that the consequence is—*the magnetic equator is one single and continuous line on the surface of the earth.* So far, then, the phenomena appear to be compatible in a rough degree with the hypothesis; and a more minute comparison cannot be fairly instituted, till more perfect data are obtained.

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* *Phil. Trans.*, 1835, p. 233.

† Ibid. p. 244, art. 12.

The remainder of these papers is devoted to the investigation of the properties of the magnetic curve, which are very fully developed: and to the determination of the curve of verticity (or that at any points of which a needle being placed, it shall be directed towards the centre of the earth, or perpendicular to the horizon), and of the number of points in which such a curve can meet the terrestrial surface. We can do little more, in a popular form, than describe the general character of the methods of inquiry, and state the result in which those investigations terminate.

He first obtains the rectangular equation of the curve of verticity*, and finds it, like the magnetic curve from which it is generated, is of the eighth degree: and that combined with the equation of the magnetic meridian †, it gives an equation of the tenth degree from which to determine either co-ordinate of the point of verticity on the terrestrial surface; and, as he remarks, after several fruitless attempts to solve these equations, or those which arose from them by transformation, he was compelled to admit that, in the present case, *fallere et fugere est triumphus.* However, by means of the polar equation, this important fact was brought out, that *no more than two points of the curve of verticity can exist in the same line drawn through the centre of the earth* ‡.

Returning to the original conditions, he then finds that two separate cases of the problem are involved in the same general equations, only one of which can belong to the physical problem: namely, when the magnetism in the two poles is of like kinds and of unlike kinds—the latter being the one which must inevitably belong to the constitution of the terrestrial magnet in the hypothesis; since it is contrary to all analogy and to all imagination, to suppose that a single magnet can have only two poles, and those both of the same kind. It required considerable and laborious investigations to separate, in a perfectly satisfactory manner, the two sets of branches of the magnetic curve involved in the general equation distinctly from each other, and assign them accurately to their respective physical cases; and it was at last effected only by the introduction of a totally new series of mathematical processes, though ultimately brought into a form adapted, in some degree, to pre-existing ones. The general method consists in an investigation of the necessary expressions for the various lines, singular points, &c., which geometers have occasion to discuss in the examination of the properties of curves, by means of the angles of the triangle, whose base is the magnetic axis and vertex of a point in the curve. From the properties of the *convergent* magnetic curve §, such properties of the corresponding curve of verticity are deduced, as are essential to the solution of the physical problem; together with the general character of that corresponding to the divergent branches: and carefully-drawn figures of them are annexed. The consequence at which he arrives is—*that if we admit the hypothesis of two centres of magnetic force situated within the earth, there will be two and only two points on the earth's surface, at which the needle can take a position vertical to the horizon.*

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* It had before been shown, and indeed a moment's reflection renders it obvious, that these loci are linear, and all situated in one plane, passing through the magnetic axis and centre of the earth.

† The great circle-plane passing through the magnetic axis.

‡ *Phil. Trans.*, 1836, p. 104.

§ The curve which is produced when the forces are of unlike kinds: the *divergent* being those which result when the forces are of the same kind. These distinctive and appropriate terms were first employed by Professor Leslie, in his *Geometrical Analysis*, page 399.

"Whether," he continues, "this be the number actually existing on the surface of our earth, we are not at present in a condition to determine. One such undoubtedly there is, and a second is probable, but its position has not been assigned; neither, from any observations yet published, can it be even approximately determined, nor, therefore, its existence positively affirmed. I am not aware that any observations give reason to suspect the existence of more than these two; and hence, so far as we can judge from the data before us, the conclusion now obtained as a consequence of two magnetic centres of force, is consistent with the phenomena for which the hypothesis is required to account. *It is, therefore, a strong argument, in the present state of our actual knowledge of the phenomena of terrestrial magnetism, for the truth of that hypothesis*[*]."

This is the utmost extent to which the solution of the problem has been advanced in this direction: and here we are compelled, for the present, to leave the subject.

ADDENDUM.—Since this paper has been in type, a memoir by Baron Humboldt has been submitted to the Royal Society, and a Report upon it, drawn up by Mr. Christie and the Astronomer Royal, has been also given in. The object is to urge civilized governments, especially that *of England*, to establish a few well-appointed *permanent* magnetical observatories in appropriate positions on the globe. Many of the difficulties to which we have alluded at p. 383, would then be removed: and we have little doubt of this system completely supplying all the data that will be required. We trust the British Government will not be backward in seconding the efforts made to complete a system of inquiries, which in origin and cultivation have hitherto been almost entirely British, and in which, too, Britain has a deeper personal interest than any other nation in the world.

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[*] *Phil. Trans.*, 1836, page 124. He remarks, however, that could we conceive such a constitution of the terrestrial magnet as should have two poles of like kinds, there would *always be two points of verticity*, and within certain limits of relation between the positions of the poles, the centre of the earth and the magnitude of the earth's radius, there *may be four* such points.

MISCELLANEOUS INTELLIGENCE.

Precision in Scientific Terms. No. III.

FORCE; POWER; FLUENT-POWER.—"A great source of confusion and error in Mechanical Philosophy, as treated of in books, is an indiscriminate and ambiguous use of the terms, *force, power, resistance,* and others of correlative import, to signify *several different sorts of quantities;* accompanied by a corresponding vagueness of apprehension in regard to the existence and nature of these differences. Whether the ambiguity of language is the *cause* or the *effect* of the indistinct notions on the subject, I need not inquire. It is certain that they tend mutually to perpetuate each other, and I have no doubt that, collectively, they have done more to retard the progress of mechanical philosophy, and to bring it into disrepute among practical men, than all other causes united. That there may be no misapprehension in regard to the precise meaning I attach to the language I use in speaking of these different quantities, I shall, before I proceed further, point out the differences to which I allude, and the mode in which I shall distinguish them.

Mechanical agency may be contemplated under several different aspects, in each of which respectively, its magnitude is a different species of quantity. My present purpose requires me to notice three of these:—

1. It may be considered with reference solely to the simple pressure or effort, exerted at any *point,* or *indivisible instant of time.* In this view its magnitude is expressed simply in pounds; this quantity I call *force,* or *force* of resistance.

2. It may be considered with reference not only to the *force,* or pressure at any point of time, but also with reference to the distance through which it is exerted, but without reference to the time occupied in moving through that distance. In this view its magnitude is expressed by the product of force and distance. The cost and value of all mechanical effects, and, of course, the cost and value of all mechanical power, are proportional to this product; for this reason I designate the quantity resulting from the product of force and distance by the name *power.* This product is the true measure of mechanical power in all cases, when contemplated as an agent, producing a *determinate* amount of any of the various ultimate effects aimed at in practical mechanics; for the amount of effect produced will always be proportional to this product in the moving power.

3. Mechanical agency may be contemplated as a quantity depending for its magnitude, not on the total amount of its effects, but on the rapidity with which it produces given amounts of effect; that is, it may be considered not only with reference to the force and distance, but also with reference to the shortness of time occupied in passing through that distance.

In this view its magnitude is expressed by the product and distance divided by time, or which is the same thing, the product of force and velocity. This product of force and velocity is not a measure of power, but of a ratio which power bears to time, or in other words, it is a measure of the rapidity with which power flows out and is brought into action. This quantity, therefore, I call *fluent-power.*

I will recapitulate these distinctions.

Force is the pressure in pounds.

Power is the product of force and distance.

Fluent-power is the product of force and velocity.

Again,—

Force is simple pressure, irrespective of duration or motion.

Power becomes developed as this force moves, in proportion to the distance moved through.

Fluent-power is the ratio of this developement to the time in which it takes place.

These are three quantities, which are totally different in their nature, and between which it is of the utmost importance clearly to distinguish, in every branch of mechanical philosophy in which they are introduced, and in none more so than in that which treats of the resistance of fluids.

It may seem incredible to some, that distinctions so obvious and simple, and the propriety, and even necessity, of which are so manifest, can need to be laid down, and insisted upon in the nineteenth century, especially in a branch of science which has been cultivated ever since the days of Archimedes. But if any one to whom this sentiment may occur, will first clear up his own views with regard to these distinctions, and will then in the light of them examine the books, he will be astonished to find what a medley of confusion and error they contain, in every branch of mechanical philosophy to which these distinctions are applicable. Even the Treatise by Olinthus Gregory, who, perhaps, brought as high a degree of mental and mathematical acumen to bear upon the subject, as any who have preceded or followed him, should not be excepted from this remark.

It was an oversight, or misapprehension, of these distinctions, that embarrassed the views of your two correspondents, to whose communications I have already referred, and which has led them to suppose that the different results at which they have arrived, in estimating the perpendicular action of a fluid on a plane oblique to the line of its motion, are at variance with each other. Correctly understood, these results are in perfect accordance, not only with each other but with the common theory. They differ, it is true, not, however, in *principle*, but only in the *nature of the quantity* deduced."

The two correspondents referred to, are two highly-respectable Professors in the United States. Mr. Blake, whose remarks we have been quoting, after stating the question between them, and showing that, by the strict use of the terms in the senses he has proposed, the debateable ground between the disputants is narrowed, if not altogether annihilated, proceeds:—

"In the foregoing remarks, I have endeavoured to give a prominence to the distinctions I have made between the different aspects in which mechanical agency may be contemplated, corresponding to my views of their importance. That the quantities between which I have endeavoured to distinguish are different, is no new discovery. Their difference has always been recognised, whenever it has been adverted to. It has, however, been so little adverted to, at least in treatises on mechanics, that it is scarcely too much to say, that it has been wholly overlooked. Nothing is more common, not only in loose conversation and writing, but even in the books which profess to *teach* on this subject, than to find these several quantities spoken of under one and the same *name*, without any discrimination at all, and evidently without any apprehension of their difference. It is to this circumstance, as I have already suggested, that I chiefly attribute the well-known fact that, in reference to the application and use of mechanical power, theory and practice have hitherto wooed each other almost in vain. Whenever a good treatise of mechanics shall be given to the public, in which these distinctions are laid down, *in limine*, as fundamental, and carried out through all branches of the subject, as I have endeavoured to carry them out here, that moment, in my view, theory and practice on this subject will be wedded, and a new era in their history will commence."—Blake, *Remarks on the Theory of the Resistance of Fluids.* Silliman's *Journal*, No. LX. 1836.

Without expressing any opinion, at present, upon the accuracy of the definitions given by Mr. Blake, it is impossible not to acknowledge the justice of his observations. The careless, and therefore, the frequently contradictory use of terms, is an old and inveterate disease. The experiment advised by Mr. B. to those who have any doubt upon the subject, must, unfortunately, under existing circumstances, be undergone by at least every self-taught student. It is a painful necessity which ought to be removed, and which would be in a short time, if authors were as anxious and as fearless to be understood as this gentleman. His example, if followed by our own countrymen, who are the greatest sinners in this respect in Europe, would save many a tiresome and unprofitable investigation and many a mischievous dispute.

Safety-Stopper for Steam-Boilers.

EXPLOSIONS of steam-boilers are frequently the effect of an accumulation of sediment within them. This forms a crust on the interior surface, particularly on the bottom, and being a bad conductor of heat, insulates the metal from the water. It is, therefore, extremely favourable to the former acquiring a high and injurious temperature. When this has happened, the difference of dilatation, which sometimes occurs, destroys the adhesion of the sediment, and exposes the incandescent metal to the water. The instantaneous production of steam, which is the consequence of the contact, is often so enormous, that the boiler, unable to withstand the sudden increase of pressure, explodes with more or less violence. To this peculiar case of danger, no one had, up to a very recent period, been very successful in applying a means either of remedy or of prevention. To use distilled or filtered water, to invite the deposit to subside in parts of the boiler not exposed to the fire, and to prevent accumulation, by frequently cleaning out the boiler, were the principal means used. That these have been very inadequate, and that a remedy for the evil, or a prevention of its occurrence, had been long desired, every one at all acquainted with the causes of destruction of steam-boilers is well aware. Water which, on evaporating, leaves no sediment, cannot be obtained in a vast majority of instances in which steam-boilers are used, and the labour and inconvenience which the frequent removal of the deposit requires, often causes this necessary operation to be deferred far beyond the time that it ought to be done, either for the preservation of the boiler, or the safety of those near it. With this view of the case, and with the recollection of the length of time this particular cause of mischief had baffled the ingenuity of the most successful improvers of the steam-boiler, we can easily feel how eagerly would be received an invention which should faithfully give an indication of this peculiar danger whenever it may be approaching, and which should, if this indication be *neglected, avert the* danger, by suspending, in proper time, the action of the *immediate agent.*

Such an invention has been recently accomplished by M. Galy-Cazalat[*]. It has been patented both in France and in England; and after the usual severe examination of *La Société d'Encouragement* of Paris has been declared worthy of their large gold medal.

The simple and ingenious apparatus, by which M. G.-C. succeeds in rendering this essential service to the safe application of steam-power, is the following:—it should, however, be premised, that the invention is not intended to be substituted for the present safety and escape-valves, but to be used in addition to them. A small tube is passed through the boiler and its top and bottom, directly over the part where the fire strikes the latter most forcibly. The upper extremity of the tube projects from the boiler, and is terminated by a transferring-cock[†]. The lower extremity is riveted to the boiler-bottom, and its aperture is slightly contracted in its diameter. In the passage of the tube through the boiler, a communication is made between them by a few holes. These are pierced in the tube, and so as to be as high as possible above the regular water-surface level.

The aperture of the lower extremity of the tube is closed by a means which deserves attention. A stopper, which may be either a plug or a ball, of fusible metal, is deposited in the transferring-cock, and by the half-turn of the handle conveyed into the tube: as soon as it has dropped from the cock, it is exposed to the current of steam rushing through the perforations in the tube, and is driven along like the ball in an air-gun. At the end it is arrested by the contracted diameter of the tube, and is so powerfully held there, that it hermetically seals the aperture. This stopper, so applied, becomes an integrant part of the boiler, and from its position is, like it, exposed to the direct action of the fire; but there is this important peculiarity with regard to the stopper—it never can be, on its upper side, in contact with anything but steam. Now that of the boiler-bottom is always intended to be in contact with water, and may be separated from it by sediment.

If we suppose a boiler-bottom, guarded by a safety-stopper, to be liable to become dangerously heated by either of the sets of circumstances which are the sole causes of this species of mischief[‡], the stopper will melt before the danger becomes serious, the tube will be uncorked, and steam will issue precisely at the point it can be most effective, *i. e.* directly upon the fire; the latter will therefore be instantly damped, and the usual danger as instantly averted. This alone is a most valuable property in the invention, and sufficient to recommend it to general adoption; but it has this additional advantage in practice; viz., that it operates without stopping the engine; that as it damps, not extinguishes, the fire, the supply of steam is moderated, and never entirely suspended, and that another stopper can, by means of the transferring-cock, be conveyed into the tube, and the aperture again hermetically sealed in a very few seconds after the fusion of its predecessor.

M. G.-C. extends the application of this principle to the sides, as well as the bottoms, of boilers, in order to prevent the mischief which results from a depression of the water-surface below the level of the flues. In this case the tube passes obliquely from the top of the boiler through it, and terminates in the side to be protected, and the plug-form of the stopper is changed to that of a ball. Should the water-depression take place, and the boiler-side become, in the absence of water, too much heated by the action of the fire, the ball will melt like the plug in the first instance, and the same desirable consequences will follow.

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[*] *Professeur des Mathématiques-Physiques* at the Royal College of Versailles.

[†] This cock is similar to the greasing-cock of a steam-engine—its plug is not perforated, but has a chamber only, this receives the article to be transferred; on turning the plug half round, the chamber is exposed to the other part of the tube, and *the article* drops into it. The use of the cock is to keep the tube closed during the *transfer*.

[‡] *These* are—total absence of water in the boiler, from deficiencies of supply, &c., *and*—*insulation* of the bottom by sediment.

M. G.-C. also suggests, that the injection-pipe of the water-supply should extend further into the boiler than usual, and be carried quite round its sides at the greatest convenient height, and that this prolongation should deliver the water through a large number of small holes, so distributed that they should bathe the whole interior surface of the sides. This would be another means of preserving them at all times from injurious increases of temperature, even when the water in the boiler might be the lowest possible.

Self-regulating Apparatus for the Supply of Steam-Boilers.

As the most frequent cause of the explosion of steam-boilers arises from the depression of the water-surface below a certain level, it is evident that the constant maintenance of this surface at a proper height, is an object of the highest importance. It appears, from causes into which we shall not now inquire, that much more attention has been paid to this essential appendage of a safe boiler in the United States and in France, than in this country. We select two instances to which publicity has recently been given, in the latter kingdom, by the *Société d'Encouragement.* One of these is of universal application, and possesses that extreme simplicity which characterizes the results of refined invention. M. Galy-Cazalat, whose safety-stopper has been described in a preceding page, is also the author of this apparatus. To the chamber of a feeding-pump (which has a power more than sufficient to supply the greatest evaporation, leakage, &c., of the boiler to which it is applied), M. G.-C. attaches a very small tube, this he leads into the boiler, and establishes a second connexion between them, the first being the usual one,—that of the injection-pipe. This small tube rises in the boiler to the required water-surface level, and there expands into a cup-shaped termination. In this lies a hollow metallic sphere, light enough to float in water; and though allowed a certain play, yet is prevented by a bridle from escaping, and thus acts as a valve. This is the whole apparatus; its action is as follows:—When the supply happens to be larger than the evaporation, &c., takes off, the water-surface naturally rises in the boiler; this rising lifts the spherical valve out of its seat, and permits a small quantity of the hot water to pass through the tube into the pump, the next exhausting stroke of which, by producing a vacuum, changes the hot water into a volume of steam, which, filling the pump, prevents the rise of water, and checks the supply. The succeeding strokes of the pump continue ineffective by the same means: they merely create and compress steam, until the water in the boiler is so much evaporated, that its surface descends low enough to reseat the spherical valve, and cut off the connexion between the boiler and the pump; this done, no more hot water can descend, the steam in the pump is condensed, and the latter recommences the water-supply; a little time may be necessary in the warming and cooling of the pump before its full operation can, in each change, be attained, but the range of water-surface in the boiler, through which it may safely travel, is always sufficiently ample to permit this. To facilitate the cooling, the piston of the pump is made hollow, so that the atmosphere may have access to it. As, in order to ensure the buoyancy of the metallic sphere, it must be made very thin, and therefore unequal to sustain great pressure when occurring on one side only, M. G.-C., to prevent its being crushed by the steam, encloses a little water within it. This is, of course, raised into steam of the same elasticity of that which surrounds it, and exactly balancing the external pressure, preserves the sphere from a change of form.

The other apparatus referred to has been adopted by the proprietor of a large establishment in France, and can be used in stationary steam-boilers only. The apparatus consists in a receiver and two stop-cocks, put in action by a counter-weight, which is raised occasionally by an hydraulic lever. A correct idea of the arrangement may be formed by imagining a cylinder to be attached to one end of the beam of a balance, and a weight at the other.

The receiver, by its position with regard to the steam-boiler and a reservoir of water, can be suddenly filled with either water or steam. Its weight is thus varied, and will become either greater or less than the counter-weight. When filled with water it is greater, and, of course, the beam declines on the side of the receiver. When this is replaced by steam, the counter-weight is heavier. This then descends, and raises the receiver. The axis of the beam is in connexion with the stop-cocks, which, in the several positions resulting from the movement of the beam, open communications alternately with the reservoir and with the boiler.

When the receiver has obtained its dose from the reservoir, it descends by its superior weight; a stop-cock is opened, and, if the water-level be low, the contents of the receiver pass into the boiler; and the former losing weight by the exchange of water for steam, is lifted by the counter-weight to the reservoir, and, receiving a further supply, again descends. If the water-level be sufficiently high, though the communication by the stop-cock be made, yet no water passes, and the receiver remains stationary until the water-level descends below the assigned point. This last effect is owing to the orifice of communication being below the desired water-level; and of course not being open unless the water-surface has descended below it.

This mode of feeding a boiler has been in actual operation, in the establishment in which it is erected, for several years, and its success has completely demonstrated the practicability and the value of the design.

Another Bequest to the Ingenious.

"To the President for the time being of the Civil Engineer Institution in Trust, the interest to be expended in Annual Premiums, under the direction of the Council, Two Thousand Pounds.

"All my scientific Books, Book-cases, Prints, and such Drawings as my Executors shall consider suitable, are to be delivered to the President of the Civil Engineer Institution, for its use and benefit, on condition that all those Articles, as well as the Books, Prints, and Drawings already presented by me, shall, in case of the said Institution being discontinued, be delivered to the Royal Society, Edinburgh, for its use."

Extract from the Will of the late Thomas Telford, Esq.,
Civil Engineer, London, who died in the Autumn of 1834.

In our last Number we inserted the noble Bequest of John Scott, of Edinburgh, who, far away from his native country, left a considerable sum of money for the encouragement of the useful arts; and we also thought it a duty to animadvert on the conduct of those parties in whose hands the distribution of the proceeds of the legacy were ultimately left. We have now recorded a later case of the same public-spirited description. Mr. Telford, also a Scotchman, also dying, though in London, yet far removed from Scotland and his native place, bequeaths also a considerable sum of money for a very similar purpose. The case is, indeed, so far so precisely parallel, that we proceeded with more than ordinary interest to ascertain the conduct of the trustees or their agents to whom the care of the latter bequest was confided. We certainly did venture to hope that the parallelism might not continue further in the two cases, but we were determined, if it did, to express ourselves as freely, at least, of the Institution of Civil Engineers of London, as we had done of the Franklin Institute of Philadelphia.

We are glad to announce, as Englishmen and as men, that our hopes were realized, and we have the gratification to subjoin, for *the information of the ingenious throughout* THE WORLD, the following resolutions of the Council, under whose direction Mr. Telford has left the interest of the £2000 to be expended.

Extract from the Minutes of a Special Meeting of the Council of the Institution of Civil Engineers, held 23rd February, 1835.

RESOLVED, upon consideration of the above bequest of their late highly-esteemed and much-lamented President, that—

I. The Premiums to be given be both of an honorary and pecuniary nature.

II. That the honorary premiums consist of medals in gold, silver, and bronze, to be called the "Telford Medals," with a head of the late president on one side, surrounded by the words, "Institution of Civil Engineers, founded 1818;" and on the other "Telford Medal," and a suitable device, leaving a space for the name of the successful candidate, and the object of the reward; or of such other description of honorary medals, and of such size and value as shall be determined by the Council.

III. That the principal subjects for which premiums will be given are—1. Descriptions, accompanied by plans and explanatory drawings, of any work in civil engineering, as far as absolutely executed; which shall contain authentic details of the progress of the work*.—2. Models or drawings, with descriptions of useful engines and machines; plans of harbours, bridges, roads, rivers, canals, mines, &c.; surveys and sections of districts of country.—3. Practical Essays on subjects connected with civil engineering, such as geology, mineralogy, chemistry, physics, mechanic arts, statistics, agriculture, &c., together with models, drawings, or descriptions of any new and useful apparatus, or instruments applicable to the purposes of engineering or surveying.

IV. No premiums can be given until the next session of the institution, but specimens of the "Telford Medals" will, if possible, be provided for the inspection of the members previous to the close of the present session; and any communications for reward, presented during the present session, will be considered as subjects for premiums in 1836.

V. The number or nature of premiums to be determined by the Council at a special meeting or meetings to be called for that purpose. No member of the Council can be present at any meeting for determining the premium of the class for which he may be a candidate. The quorum of the council for deciding on the premiums must consist of at least the president, two vice-presidents, and four members, or in case of the unavoidable absence of the president, of three vice-presidents, and four members of the council; being in either case seven, as the smallest number of a Council for awarding premiums.

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* *Smeaton's* account of the Edystone Light-house may be taken as an exampl

VI. The premiums to be distributed to the successful candidates at a special general meeting at the end of the session.

VII. In the distribution of premiums no distinction will be made between natives and foreigners.

VIII. The proceedings in Council respecting the premiums to be made known at the ordinary meeting of the 24th of February, from the chair; and that printed copies of the same be circulated to every member of the Institution, together with the substance of the address of the President on the 27th of January, 1835.

To the bequest of Mr. Telford and these resolutions, *and particularly to the seventh,* we desire the same extensive attention that we requested to the legacy of Mr. Scott, and to the terms in which it was actually bequeathed; and further we appeal to the justice of the Franklin Institute, that they will enable us soon to announce that the following statement and contrast has ceased to be true.

The proceeds of a legacy of £530 were left to " ingenious men and women," by a Scotchman dying in America. The Franklin Institute of the state of Pennsylvania for the promotion of the mechanic arts, endeavour to limit the knowledge and the advantage of the bequest to " the ingenious of the *United States* only."

The proceeds of a legacy of £2000. were left " to be expended in annual premiums" by a Scotchman dying in London. The Institution of Civil Engineers of London, at liberty to make any restriction or condition they please, take no advantage of their position, but decide that in the distribution of the premiums " no distinction shall be made between *natives and foreigners.*"

Estimate of the Efficacy of the Hot-Air Blast.

THE calculations, &c., of M. Clement Desormes, have induced him to conclude that the temperature of the furnace for smelting iron is increased 270°,—360° Fahr., by the introduction of the hot-air blast; and he states, that he considers this increase to be adequate to the explanation of all the observed effects.

Estimate and Expression of the Value of Acclivities in Roads.

IT is evident that, if it were possible, by calculation and the correct reduction of the various kinds of difficulties into equivalent quantities of the same kind, the comparison of two lines of road, of whatever material or construction, might be simply and satisfactorily expressed, either by numbers, or (to the eye) by lines of proportionate length. Supposing, for example, there were two roads leading from the same commencement* to the same terminus, and that it were wished to express, as concisely as possible, the merits of each, could it be done better than by saying the balance of the advantages and disadvantages of the one, compared with the other, are as (suppose) 5 to 4; or more extendedly, that 50 miles might be run in the same time, with the same load, as 40 miles on the other; or supposing one line to be entirely level, and the other has depreciating acclivities, if it were said that 90 feet of rise in the latter were equivalent to a mile of distance on the former, &c.? The quackery and humbug of such terms as "characteristic gradient" would then, as they ought, be banished from all rational discussion. Nothing can be more amusing or pitiable than the frequent misapplication of the barbarous term "gradient," both by engineers and legislators, in the present parliamentary committees. The invention of this term, it is said, originated with a drunken Irishman (an engineer!), who endeavouring to give the proprietors of a road, in a hilly district, some excuse for an excess of cost over estimate, hiccupped out "that the rise over —— Hill must be an *in—in—n—n—gradient* in their calculation." "Is it in gradient you *mane?*" said the chairman. "It—it is," said the engineer.

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* *In the* parliamentary engineering language of the present session, there is no word *expressive of the* commencement or starting point of a rail-road, nothing but "*terminus*" *and* "*termini,*" are yet provided. So that English gentlemen and English counsel are *constantly* talking about " beginning at the end (terminus) of such and such a railroad!"

When the reasoning and formula given by Navier, in his Essay on the comparison of competing Lines of Railroads[*], are better understood, the jargon and mystification we complain of will be unknown, and common sense may preside unruffled in committees, &c., on engineering subjects.

Railroad Acts, present Session, (June 24th incl.)

THE following additional Railroad Bills have received the royal assent:

10. June 7. Brandling Junction.
11. — 21. London & Dover (South-eastern.)
12. — „ Newcastle and North Shields.
13. — „ Cheltenham and Great Western.
14. — „ Midland Counties.
15. June 21. Birmingham, Bristol, and Thames Junction.
14. — „ Hull and Selby.
15. — „ York and North Midland.
16. — „ Merthyr Tydfil and Cardiff.
17. — „ Deptford Pier Junction.

British Association.—Sixth Meeting.

THE sixth meeting of the British Association for the Advancement of Science, is definitively announced to commence at Bristol, on Monday, the 22nd of August. It will continue during the whole week. The officers of the association assemble on Saturday, the 20th of August, to make the necessary arrangements. They must work on Sunday, if Monday and part of Tuesday are not to be lost to the members as usual.

Patent Law Improvement Bill.

THE following notices appear in the printed votes of the House of Commons. We have searched the journals of the public press in vain for any notice of them, or for further information relating to their important object. The bill itself is not yet (June 25) printed for public circulation.

June 14th, 1836.—"Act (5 and 6 Will. IV., c. 83[†]) read. Bill to amend the said act, ordered to be brought in by Mr. Mackinnon[‡] and Mr. Hardy[§]."

June 20th, 1836.—"Bill 'to amend the law relating to Letters Patent for inventions, and for the better encouragement of the arts and manufactures,' *presented* and read 1°; to be read 2° on *Wednesday,* 29*th* June, and to be *printed.*"

A change seems to have taken place in the intended parliamentary proceedings on this subject. Whether Mr. Mackinnon asked for the committee for which he had given so long notice, we cannot learn; but it seems now he has passed that point, and obtained leave to bring in a bill. The title of this also varies considerably from the words of the notice; it is more comprehensive and general. To this there can be no objection; but past experience, on this very question, induces a fear, that by grasping at too much, little or nothing may be obtained; and that the very desirable reforms of the patent law, specified in the notice may be neglected, or omitted, among the numerous provisions which must necessarily be subjects of discussion in a satisfactory bill for the better encouragement of the arts and manufactures.

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[*] Lately translated by Mr. Macneill, see p. 60 of the present volume.

[†] Given at length at p. 133 of the present volume of this Magazine.

[‡] Wm. Alex. Mackinnon, M.P. for Lymington, 4, Hyde-Park Place.

[§] *John Hardy,* M.P. for Bradford, 7, Portland-Place.

Patent Law Grievance. No. IV.

THE penalties inflicted on the inventive genius of Britain during the present year, up to the 25th ult., in the shape of government stamps and fees on patents, amount to more than £21,000!

N.B. This sum has been paid in *ready money*, on taking the first steps, and as many of the inventors are poor men (operatives), and a great many others of them persons to whom it would be very inconvenient to pay at least £100. down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c.

NEW PATENTS. 1836.

ENGLISH.

N. B.—The first Date annexed to each Patent, is that on which it was sealed and granted; the second, that on or before which the Specification must be delivered and enrolled.——The abbreviation *For. Comm.*, signifies that the invention, &c., is "a communication from a foreigner residing abroad."

GRANTS.

MAY *contd.*

130. JOSEPH BENCKE GEROTHWOHL, Camberwell-grove, *Surr.*, Merchant; for improvements in filtration. May 28.—Nov. 58. *For. Comm.*

131. FRANCIS PETIT SMITH, Hendon, *Mid.*, Farmer, for an improved propeller for steam and other vessels. May 31.—Nov. 30.

TOTAL, MAY...29.

JUNE.

132. WILLIAM GOSSAGE, Stoke Prior, *Wor.*, for improvements in the apparatus, or means, used for evaporating water from saline solutions, and in the construction of stoves for drying salts. June 2.—Dec. 2.

133. LUKE HEBERT, Paternoster-row, *Lond.*, Patent Agent, for improved machinery and processes for economizing and purifying the manufacture of bread, a part of which is applicable to other purposes. June 2.—Dec. 2.

134. BARON HENRY DE BODE, Maj. Gen. Russian service, Edgware-rd., *Middx.* for improvements in capstans. June 4.—Dec. 4.

135. MANSAH BOWER, Birmingham, *War.*, for improvements applicable to various descriptions of carriages. June 7.—Dec. 7.

136. JOHN YOUNG, Wolverhampton, *Staff.*, Patent Lock-smith, for improvements in the making or manufacturing of metal hinges for doors and other purposes. June 7.—Dec. 7.

137. DANIEL CHAMBERS, Carey-st., Lincoln's Inn, Water-closet manufacturer, and JOSEPH HALL, Margaret-st., Cavendish-sq., Plumber, for an improvement in pumps. June 7.—Dec. 7.

138. MILES BERRY, Chancery-lane, *Middx.*, Mechanical Draftsman, for improvements in machinery, or apparatus for cleaning, purifying, and drying wheat. or other grain or seeds. June 7.—Dec. 7.

139. AMOS GERALD HULL, Cockspur-st., Charing-cross, *Middx.*, Esq., for his invention of improvements in instruments for supplying the prolapsed Uterus. June 9.—Dec. 9.

140. Edward Massey, King-st., Clerkenwell, *Middx.*, Watchmaker, for improvements in the apparatus used for measuring the progress of vessels through the water, and for taking soundings at sea. June 13.—Dec. 13.

141. Jacob Perkins, Fleet-st., *Lond.*, Civil-engineer; for improvements in apparatus for cooking. June 13.—Dec. 13.

142. Miles Berry, Chancery-lane, *Middx.* Civil-engineer; for improved apparatus for torrefying, baking and roasting vegetable substances; which with certain modifications and additions is also applicable to the evaporation and concentration of saccharine juices and other liquids. June 13.—Dec. 13. *For. Comm.*

143. Alexander Ritchie, Leeds, *York*, Merchant; for an improvement in dressing and finishing woollen cloths, and other woven fabrics. June 13.—Dec. 13.

144. Charles Schafhautl, Dudley, *Worc.*, Gent.; for improved apparatus for puddling iron. June 13.—Dec. 13.

145. Thomas Vaux, Woodford-bridge, *Essex*, Land-Surveyor; for a mode of constructing and applying a revolving harrow for agricultural purposes. June 13.—Dec. 13.

146. John White, *Southampton*, Engineer; for improvements on rotary steam-engines, which implements or parts thereof are applicable to other useful purposes. June 15.—Dec. 15.

147. James Dredge, Bath, *Som.*, for improvements in the construction of suspension chains for bridges, viaducts, aqueducts, and other purposes, and in the construction of such bridges, viaducts, or aqueducts. June 17.—Dec. 17.

148. John Hopkins, Exmouth-st., Clerkenwell, *Middx.*, Surveyor; for improvements in furnaces for steam-engine boilers and other purposes. June 18.—Dec. 18.

149. Louis Gachet, Cambridge-Heath, *Middx.*, Gent.; for improvements in machinery for manufacturing and producing velvets and certain other fabrics. June 18.—Dec. 18.

150. Joseph Bunnett, Newington-causeway, *Southwark*, Window-blind maker; for improvements in window-shutters, which improvements may also be applied to other useful purposes. June 18.—Dec. 18.

151. William Watson, Liverpool, *Lanc.*, Merchant; for improvements in the manufacturing of sugars from beet-root, and other substances. June 18. —*Dec. 18.*

152. John Young, Wolverhampton, *Staff.*, Patent Lock-Smith; for improvements in manufacturing boxes and pullies for window-sashes and other purposes. June 21.—Dec. 21.

153. Robert Smith, Manchester, *Lanc.*, Engineer; for improvements in the means of connecting metallic plates for the construction of boilers and other purposes. June 22.—Dec. 22.

154. William Wright, Salford, *Lanc.*, Machine maker; for improvements in twisting machinery used in the preparation, spinning or twisting of cotton, flax, silk, wool, hemp, and other fibrous substances. June 22.—Dec. 22.

155. Charles Pearce Chapman, Cornhill, *Lond.*, Zinc manufacturer; for improvements in printing silks, calicoes, and other fabrics. June 22. —Dec. 22.

156. William Bamatt, Brighton, *Sussex*, Founder; for improvements in apparatus for generating and purifying gas for the purposes of illumination. June 22.—Dec. 22.

157. Hamer Stansfeld, Leeds, *York*, Merchant; for improvements in machinery for preparing certain threads or yarns, and for weaving certain fabrics. June 22.—Dec. 22.

158. John Woolrich, Birmingham, *Warw.*, Professor of Chemistry; improvements in producing or making the substance commonly called, or known by the name of, carbonate of baryta or carbonate of barytes. June 22.—Dec. 22.

159. Henry Dunnington, *Nott.*, Lace-manufacturer; for improvements in making or manufacturing lace. June 22.—Dec. 22.

160. John Mc Dowall, Johnstone, *Renfrew*, N. Brit., and of Manchester, *Lanc.*, Engineer; for improvements in the machinery for sawing timber, and in the mode of applying power to the same. June 24.—Dec. 24.

161. George Richard Elkington, Birmingham, *Warw.*, Gilt Toy-maker; for an improved method of gilding copper, brass, and other metals, or alloy of metals. June 24.—Dec. 24.

162. Samuel Hall, Basford, *Nott.*, Gent.; for improvements in propelling vessels, also improvements in steam-engines, and in the method or methods of working some parts thereof, some of which improvements are applicable to other purposes. June 24.—Dec. 24.

163. Alexandrr Stocker, Birmingham, *Warw.* Gent.; for improvements in machinery for making files. June 25.—Dec. 25.

METEOROLOGICAL JOURNAL FOR MAY, 1836; KEPT AT BLACKHEATH ROAD.

Day of Month		Barom. 9 A.M.	Ther. attch.	Barom. 3 P.M.	Ther. attch.	Thermometer Min.	Thermometer Max.	Daily Temp.	Solar Var.	Rad.	Clouds. A.M.	Clouds. P.M.	Wind. A.M.	Wind. P.M.	Direction of wind A.M.	Direction of wind P.M.	Lunation.	WEATHER, &c.
SUN.	1	29·931	50°	29·935	52°	31°5	50°6	41·0	19°1	29°	5	8	3	3·4	N.E.	N.E.	○	Cold wind; *cumulus* & *nimbus*, with squalls of sleet.
Mon.	2	29·975	52	30·040	54	38·6	54·0	46·3	15·4	38	10	8	5	4	N.E.	N.E.		A heavy gale of wind with rain; inclement and cold.
Tues.	3	30·100	51	29·995	53	39·1	51·5	45.3	12·4	38	10	9	4	3	N. b E.	N. b E.		Wind very high; rainy evening; ditto.
Wed.	4	29·824	52	29·804	54	43·0	55·0	49·0	12·0	43	10	6	2	2	N.E.	N.E.		Overcast, with showers.
Thurs.	5	29·808	53	29·902	54	41·2	54·5	47·9	13·3	41	10	9	2	2	E. b S.	E.		Heavy showers; clear from 8 to 11 P.M.; *stratus* at night.
Friday,	6	30·192	53	30·230	55	40·9	57·0	49·0	16·1	40	7	4	2	2	E.N.E.	N.E.		*Cumuli*; air fine and drier; clear night.
Satur.	7	30·411	55	30·392	56	40·0	58·2	49·1	18·2	38	0	0	3	2	N.E.	N.N.E.		Fine and clear, A.M.; cloudy night; cold wind.
SUN.	8	30·400	56	30·355	57	42·5	58·3	50·4	15·8	42	2	3	2	2	N b E.	N.E.	☾	Fine; scud; windy; *cirrus*.
Mon.	9	30·378	57	30·350	58	42·0	57·1	49·6	15·1	40	3	2	3	2	N.N.E.	N.N.E.		Ditto ditto ditto.
Tues.	10	30·339	56	30·314	59	42·2	58·2	49·2	18·0	38	5	2	2	2	N.E.	E.N.E.		Cloudy at sunrise; fine and clear throughout.
Wed.	11	30·345	55	30·274	61	33·0	64·9	48.9	31.9	31	3	2	1	1	S.W.	W.		Fine; *cirrus*, with haze.
Thurs.	12	30·346	62	30·350	63	41·9	67·0	54·4	25·1	38	0	0	1	1	W.	W.		Cloudless.
Friday,	13	30·480	63	30·482	64	40·3	66·5	53·4	26·2	37	0	0	1	0	W.	N.W.		Ditto.
Satur.	14	30·665	62	30·680	64	42·0	66·8	54·4	24·8	40	0	0	1	1	N.N.E.	E.		Clear.
SUN.	15	30·700	63	30·650	66	40·6	66·0	53·3	25·4	38	0	1	0	1	N.N.E.	N.E.S.E	●	Ditto; a few scattered clouds; *cumuli*; a haze to the westward.
Mon.	16	30·665	64	30·606	66	43·5	69·1	56·3	25·6	40	0	0	1	2	N.E.	N.E.		Ditto ditto.
Tues.	17	30·652	64	30·580	66	44·0	69·5	56·7	25·5	41	1	1	2	2	E.	E.		Ditto ditto.
Wed.	18	30·500	63	30.431	66	47·2	65·0	56·1	17·8	44	2	2	2	2	E.	E.		*Stratus*, A.M.; very clear, P.M.; a *stratus* at night.
Thurs.	19	30·426	60	30·334	65	48·0	59·2	53·6	11·2	46	7	0	2	2	E.N.E.	E.		Scud, A.M.; ditto; *Aurora Borealis* at night.
Friday,	20	30·218	61	30·126	66	41·3	66·8	54·1	25·5	38	4	0	1	2	E.	E.		Ditto ditto ditto.
Satur.	21	30·132	62	30·150	64	48·5	62·0	55·3	13·5	43	5	0	2	2	N.E.	E.		Rain fell early, A.M.; fair; cold wind and haze.
SUN.	22	30·112	62	30·045	62	39·1	54·1	46·6	15·0	35	2	0	2	2	E.	E.	☽	Windy, with loose scud.
Mon.	23	29·984	61	30·030	61	41·4	58·0	49·7	16·6	39	5	9	3	3	N.E.	E.N.E.		Rain at seven A.M.; evening and night overcast.
Tues.	24	30·215	60	30·251	60	46·5	58·5	52·5	12·0	43	5	3	2	2	N.E.	N.N.E.		*Cumuli*; mostly clear; cold.
Wed.	25	30·381	58	30·350	60	41·5	59·4	50·4	17·9	38	9	2	2	2	N.E.	N.E.		Ditto ditto.
Thurs.	26	30·440	59	30·451	60	42·5	57·0	49·7	14·5	40	3	0	3	3	E.N.E.	E. b N.		Clear sky with wind.
Friday,	27	30·528	58	30·501	60	38·0	58·8	48·4	20·8	34	1	0	3	2	E.	E.		Hoar frost at sunrise; cloudless.
Satur.	28	30·502	59	30·435	60	35·6	62·5	49·1	26·9	31	1	5	2	2	N.E.	E.N.E.		Ditto; cold dry wind; *cumuli*.
SUN.	29	30·439	59	30·420	61	46·1	64·0	55·0	17·9	43	3	0	3	3	N.E.	N.E.		Ditto ditto ditto.
Mon.	30	30·415	58	30·320	61	41·0	65·2	53·1	24·2	36	3	4	2	2	N.E.	N.E.	○	Ditto ditto ditto.
Tues.	31	30·225	59	30·109	62	47·0	68·0	58·5	21·0	42	5	3	2	2·3	N.E.	E.		Sky turbid, A.M.; clouds; evening overcast.
Mean		30·311	58	30·287	60	41·54	60·74	51·12	19·20									

Bar. Max. 30 in. ·725 on the 14th. || Ther. Max. 69°5 on the 17th. || Lowest point of Rad. 29°, on the 1st.
Bar. Min. 29 in. ·800 5th. || Ther. Min. 31°5 1st. || Rain fallen 1 in. ·220.

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